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Franklin Leonard Pope

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VOL. V.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. V. New York, October, 1887. No. 1.

SPECIAL MEETING.

Held at 136 Fifth avenue, New York, September 20th, 1887.

The President, Mr. T. C. Martin in the chair:

The PRESIDENT:—It is with much pleasure that we resume our meetings for the coming season with a paper so important and unquestionably so interesting as that to which we are about to listen. The attendance this evening is large, and in that respect very gratifying. We are not merely a New York society, but on the other hand our membership extends all over the country. At the present time we have members in between thirty and forty States and territories in the Union, and no fewer than ten countries of Europe, the West Indies, South America, and Canada. Under those circumstances it can be imagined that it is rather difficult to get all our members together when a paper is read here, no matter how important it may be.

The subject before us this evening is "Electric Street Cars, with special reference to methods of gearing." Mr. Reckenzaun, our distinguished guest, as you are all aware, has made a special study of this branch of the electric moter question, and it was through his genius and ability that the remarkable trip with an electric yacht was made across the English channel, the first of the kind on record. Not long after that trip, Mr. Reckenzaun presented a paper before the London Society of Arts on the subject, somewhat similar to that which he brings before us this evening, and so meritorious was it, and such was the interest which it excited, that the society felt justified in awarding Mr. Reckenzaun its silver medal. Mr. Reckenzaun leaves us to go to England to receive that medal. (Applause.)

In bringing this subject of electric cars before us this evening, it was thought possible that the paper would cover the whole subject, but after a little consultation Mr. Reckenzaun determined to restrict it more specifically to the subject of gearing, and I think you will all agree with me that there is no more important department of the subject of electric locomotion than that.

I have now the pleasure, gentlemen, of introducing to you Mr. Reckenzaun. (Applause.)

ON ELECTRIC STREET CARS, WITH SPECIAL REFERENCE TO METHODS OF GEARING.

BY A. RECKENZAUN, C. E.

Scarcely nine months have passed since your esteemed president, Mr. T. C. Martin, presented a paper on "Electric Street Cars" to the American Institute of Electrical Engineers. I have read this excellent paper with interest, and I have learned a great deal from it, as also from the discussion which followed. Mr. Martin compiled some very important statistics regarding the number of horse-cars in actual use in the United States, and he computed them to number 25,000, for the propulsion of which no less than 120,000 horses are requisite.

These figures filled me with wonderment and surprise, and when I read further that over 20,000 of these noble animals are rendered useless every year with overwork, in street railway service, I felt it to be the duty of every engineer, electrical or mechanical, to assist with every means in his power in the emancipation of the horse from such cruel service. The percentage of loss given in the paper referred to agrees closely with our European experience. We know that the natural life of a street-car horse is reduced to about four years, although it never works more than four hours a day. It seems barbarous, therefore, to employ animals for such purposes; yet until recently no economical substitute could be found. Steam, compressed air, gas and spring motors have been tried and found inefficient. Improvements may be made in one or more of these, but at the present moment the field of enterprise is entirely open to the cable and to electricity. There are over 80 miles of cable roads in operation in the United States, more than half of which have been laid down within the last five years, and there are indications that further and rapid progress will be made with this mode of traction, particularly in hilly districts, where it is impossible to apply self-propelled cars, owing to the want of cohesion between the wheels and rails when the angle of repose is approached.

On ordinary streets with gradients of not more than five per cent., electric traction will doubtless be more economical and far more convenient than cables, for well-known reasons. Not only in the first cost of construction far cheaper than the cable and its conduit, but the efficiency of the electric system is much greater.

It is admitted that on cable tramways some 70 or 80 per cent. of the total available power is consumed in dragging the cable itself, leaving a maximum of 30 per cent. for moving the cars, whereas with electric transmission over 50 per cent. can be utilized, as will be seen by the following figures, which I have obtained from experiments on the electric railway at Frankfort in Germany. This line is worked by the overhead conductor system. Its total length is four and one-tenth miles, double track and grooved rails. The entire circuit of the conductor is eight and one-fourth miles long, and its electrical resistence was calculated to be 1.6 ohms.

EFFICIENT AND WORKING COST.

During the experiments referred to, two cars were propelled at a speed of seven and a half miles an hour; one of these cars had an electric motor, and the other was an ordinary car attached to the first, and the entire weight was ascertained to be 8.35 tons, including passengers. Electrical measurements were taken from the dynamo at the power station simultaneously with those on the car. The rails were in their usual dirty condition. When running on the level road the energy used on the cars was 3.87 horse power, and on the generating dynamo 6.47, giving an efficiency of 59.82 per cent.; when travelling up a grade of 1 in 45, the motor gave eight horse power and the dynamo 13.5, which again corresponds to 59 per cent of the available electric energy. These measurements do not include the losses in the dynamo and motor, but it must be remembered that only two cars were on the road, whilst the capacity of the plant at the central station was sufficient for 14 cars. With 12 cars running under the ordinary circumstances of traffic, the steam engines were indicated, and this showed a maximum of 164 horse-power, consuming two and three-fourth tons of coal per day of 15 hours, or two and one half pounds per indicated H. P. per hour. Reduced to car miles, the weight of coal came to 7.5 pounds, which, at three dollars a ton, would cost one cent per mile run.

The Frankfort Electric Railway has carried over 3,000,000 passengers since its opening in April, 1884. The working expenses in the year 1886 amounted to $24,904, including the following items:

Salaries of directors, superintendent, engineers, electricians, conductors, drivers, firemen, and laborers	$13,615
Repairs of roadway. rails, cars, machinery, and buildings	5,088
Fuel (coals and kindling wood)	3,955
Oil and waste	831
Lighting and heatings of buildings, lighting of cars and streets	1,415
	$24,904

The rolling stock consisted of 14 cars, all of which were used on Sundays and holidays, but only eight cars were usually running on week days. If we take an average of 19 cars, then the entire expenses would amount to $6.82 per car per day, but not more than one-third of this, or $2.27, can be reckoned for the item of traction, which should only include the wages of engineers, electricians, firemen, cost of coals, oil, and repairs to machinery.

There are about a dozen electric railways in successful operation in Europe, some more extensive than the one I have just described, some smaller; but this one in Frankfort is worked at a greater expense than any of the others I know of; it has been in continuous use for nearly three and a half years, and I thought that by giving you detailed and exact figures of *one* line, I would serve your purpose better than by attempting to crowd a few generalities of all our electric tramways into the small compass of a short paper. You can take this as a safe standard of comparison with other systems of street car traction.

It will be observed, with justice, that the dynamos and motors of the Frankfort line, which was projected more than four years ago, do not yield as high an efficiency as more recent machines. This would leave us another margin. It is well to have a margin in all our estimates, for there is nothing that retards the progress of a novel enterprise more than exaggeration of facts, or statements which cannot be fully borne out in practice.

Some people are apt to base their calculations and estimates upon theoretical conditions. We should never assume perfection in the case of street railway tracks, but on the contrary we ought to adapt ourselves to the roughest of the rough in order to succeed.

TRACTIVE FORCE AND MECHANICAL RESISTANCES.

We know from experience that street rails which are exposed to dirt, grit, stones and the like, offer a resistance of at least 20 pounds per ton with the wheels in motion. Taking the loaded car at six tons, and the speed at six miles an hour, we have to exert 1.92 horse-power at the driving wheels, neglecting friction of gearing and losses in the electric motors. On narrow grooved rails, as used in England, we allow a tractive force of from 25 to 30 pounds per ton. The high resistances are readily accounted for, when it is considered that the flanges of wheels frequently take a bearing on the dirt in the grooves; at the same time the wheel tires may roll on the surface of the rails, when a regular grinding action is produced. Furthermore, it occurs frequently that one wheel is running on its proper bearing surface, whilst the other, belonging to the same axle, has its flange touching the bottom of the groove, when by virtue of different circumferential velocities, additional friction is introduced. Sometimes the rails are a little out of gauge. All these points increase the tractive force requisite with grooved rails. Here in America you have few if any such rails, consequently the facilities are superior; but your rails are not so strong as those used in England and on the continent of Europe, and the foundations are less substantial. The joints are also somewhat irregular, and these faults render the motion of a car very unsteady. A ride on some of the roads in Philadelphia reminds one of a voyage on the ocean when the sea is a little rough. Then you crowd very often 50 passengers into a car which is supposed to hold about 30. All these circumstances compel us to provide plenty of power in order that every obstacle can be overcome.

The force necessary for starting a car is of course much greater than the force required for maintaining a uniform speed, and experiments show that this initial effort amounts to three times and even four times the tractive force of the car in motion. In dry weather, on curves of 50 feet radius, the resistance to traction is doubled, and on those of 35 feet radius trebled; but when the rails are wet, this resistance, due to curvature, is considerably reduced. With a short wheel base, or, better still, with small trucks or radiating axles, the motion on curved rails is very much easier.

GRADIENTS.

The effect of gradients is a matter of simple calculation, and it must be provided for in designing and constructing a motor. We have calculated above, that the minimum effective power of a six ton car running at six miles an hour on the level is nearly two horse-power, exclusive of gear friction or any other waste. If we have to overcome a gradient of one per cent., for instance, the resistance due to gravity will be 22.4 pounds for each ton, or 134.4 pounds for the loaded car. Therefore we shall require about four horse-power to pull this car up a grade of 1 in 100. With a two per cent. grade we must have about three times the power used on level roads, and roughly speaking each per cent. of grade involves as much energy as the propulsion of a given weight over a horizontal road, if the speed is to be kept constant. On steep grades, however, it is customery to run a little slower, whereby some of the energy is saved, but for a car of the above weight, if it has to overcome grades of four per cent. it will not be safe to have less than eight horse-power available upon the driving axles. In addition to this the motor has to supply the power which is unavoidably wasted in the gearing used for transmitting motion from the armature shaft to the car axle. An ordinary street car wheel of 30 inch diameter has to revolve at least 77 times in the minute, in order to traverse a distance of seven miles an hour. Up to the present time no one has succeeded in constructing an electric motor of reasonable dimensions, weight and efficiency, to work at such a low speed. Most of the existing motors revolve at from six to twelve times the above velocity ratio, therefore it became necessary to introduce speed-reducing appliances between the motor and the driving axles.

GEARING.

The problem of devising suitable gearing for street cars carrying their own motor has been and is still one of the greatest importance, and I propose now to offer a few remarks with reference to this subject, by citing some examples which came under my own observation. The conditions to be satisfied are by no means simple, and it is somewhat difficult to arrange them in their order of merit. Most engineers endeavor to retain the recognized dimensions of street cars, because great deviations therefrom involve many inconveniences, and it is wise to adhere as much as possible to the rules which have been established by custom, convenience, and general utility. The fact that most of the street cars in all parts of the world are similar in shape, indicates forcibly that a particular pattern satisfies the demand. When taking that popular pattern of car and adapting to it our electrical and mechanical apparatus, we find that the space at our disposal underneath the floor and between the axles is meagre and extremely limited. Within this small compass we have to fix the gearing which has to give the highest possible return of a given power; it has to be strong, durable, yet light in weight; noise and vibration have to be carefully avoided; the mechanism must be simple, certain in its action, protected from wet and dirt, and finally of reasonable cost. How to combine all these virtues has been the study of every engineer who has made electrical traction his serious study.

STEEL SPIRAL CORDS.

In regard to my own observations, one of the prettiest kinds of gearing in actual use is that on one of the cars of the Berlin Lichterfeld Electric Tramway, which was constructed six and a half years ago, by Messrs. Siemens and Halske. This car has a capacity for 24 passengers; it weighs when empty 3.2 tons and makes a speed of 12 miles an hour. The electric motor is fastened under the floor in the center of the car, having its shaft parallel to the axles. On the motor shaft is a small cast iron pulley which has 27 V-grooves on its rim, and within these grooves work an equal number of spiral wire cords, which transmit the motion to both axles of the four-wheeled car. The axles carry the large pulleys, one of which has 13 V grooves on its circumference, and the other 14 grooves corresponding with those on the pulley on the motor shaft. The wheel base is five feet nine inches, consequently there is only a distance of two feet ten and a half inches between the centers of the connected pulleys.

The cords are made of a pair of steel wires wound closely upon a mandrel of about one-eighth of an inch in diameter, and when the mandrel is withdrawn there remains a stiff, yet flexible spiral, whose external diameter is scarcely [illegible] of an inch. When cut to the proper length these cords are stretched over the grooves and connected at the ends by means of steel eyes and a steel link. Experiments made with this gear showed that eight spiral cords suffice for transmitting the power to the axles when the car was full of passengers, and as there are actually 27 cords the margin of safety was deemed more than sufficient. It is a pleasure to ride on this car; there is no noise or vibration, and I was told that the spirals last a long time if carefully adjusted; but this method of transmission is not suffi ciently positive to warrant its application on steep grades. On this line there is no grade of over one per cent. rise.

CHAIN GEARING.

The second car on the Lichterfeld tramway, which, by the way, must be regarded as the oldest line in the world, as far as continuous successful operation is concerned, has chain gearing. The motor shaft is provided with a small toothed wheel, and one of the axles carries a large wheel with teeth to correspond to the gaps in the pitch chain links. This mode of transmission causes some noise and vibration, and this car runs slower than the other with the same amount of energy. This is accounted for by the greater friction on the surface of the teeth, as well as by the bending and unbending of the link joints; there is, besides, a constant probability of the comparatively heavy chain being thrown off tangentially by centrifugal force. But this kind of gearing furnishes an absolute or positive motion, which cannot be relied on by methods depending upon frictional adhesion alone.

Some four years ago, I applied two pitched chains, four toothed wheels and countershaft for propelling a heavy storage battery car in London. The chains were of steel and substantially made; the wheels of phosphor bronze correctly shaped. I found that the chain on the fast-running, first motion shaft, rattled considerably. This chain stretched a little, when the links began to ride upon the points of the teeth, and eventually broke the teeth of the small wheel on the armature shaft. Previous to that I employed some other gear, which will be mentioned shortly.

Mr. Trail, the manager of the Portrush Electric Railway in Ireland, has been more fortunate than I with chain gear, but he is not perfectly satisfied, and he is now experimenting with something else. Pitch chains are also used on the Bessbrook-Newry Electric Railway and on the Ryde Pier cars. Mr. Holroyd Smith adopted this mode of transmission on the first cars at Blackpool, in England, but he has since changed it to worm gearing, with which he is better satisfied.

CONICAL DISKS AND CHAINS.

About five years ago, I had the idea that it might be advantageous to vary the speed of a car mechanically without interfering with the motor or even the supply circuit. I took out a patent for this in 1882, and applied it to the propulsion of a large street car. The motor was carried by a frame supported upon the axles by bearings and springs, to counteract the effects of vibration. By this arrangement the whole apparatus was rendered independent of the car body, and it could be easily detached. The speed pulleys consisted of conical disks, which, when placed face to face on a shaft, each pair formed between them a grooved pulley, and by shifting these disks on the shaft towards or from each other, the said groove could be made wider or narrower, as desired. Two pairs of disks on different shafts were connected by means of a chain, the links of which were surrounded by wedge-shaped pieces of leather. The cross section of this chain band was thus shaped in order to fit into the V-grooves offered by the cones. When the said disks were pressed closely together, then the band assumed a position near the circumference of the disks, but when the latter were separated, then the groove widened, the band changed its position and run on some portion of the conical surfaces near the center of the pulley. Thus the effective or acting diameter of the first motion pulley was varied at will by pressure, and the cones on the second motion shaft followed suit automatically, because the length of the band was constant, and the second pair of cones was pressed together by a strong spring. This arrangement looked very nice, but its efficiency was low; there was a great waste of power through friction, and we finally abandoned it. It worked silently; it was strong and took up little room. The band slipped often when the car was starting, or mounting an incline.

STRAPS AND PULLEYS.

Mr Volk, of Brighton, England, employs leather link belts with great success, for transmitting the power of the motor to the car axle. This railway has been in operation since August, 1883. Mr. Volk tried first, single leather belts, but they broke every day or two ; then he used double belts with little better results, and some three years ago he adopted the leather link belt, which has given entire satisfaction. I have seen one of the latter which has driven a car over 50,000 miles and it was not yet worn out. These belts stretch when first put on, the bearings of the countershaft are therefore made adjustable by means of a slide whereby the slack may readily be taken up. When starting the car the belts slip a little, but this is not considered a disadvantage. There are no curves on the Brighton line, and I often wondered whether this method of gearing would satisfy the terrible pull on the very sharp curves which occur on some street railways.

Mr. Van Depoele has given a high testimony in favor of link belts of special construction after using them for a considerable time and running them at a great velocity. Like Mr. Volk, he attaches the motor to the car body in order to give it the benefit of the existing flexibility of the car springs. Mr. Van Depoele's link belts, I understand, are made of steel, with special bearing surfaces; they have been used on a line of his construction at Windsor, in Canada; at Scranton, Pa., he employs spur gearing.

SPUR GEARING.

There exists a variety of opinions on the subject of toothed wheels in connection with electric motors, and each opinion seems based upon actual experience. I have seen spur gearing used on electric cars in Europe and in this country, and I give you freely all I know about it. The first I saw was on the Moedling line, near Vienna, in Austria. The noise created there was not very great, but there was occasionally some trouble due to the untimely deterioration of the teeth. This was especially remarked by the manager of the Frankfort Electric Railway in Germany, which is constructed on precisely the same principles, with the difference that the former runs through a country district where there is much less traffic, and the rails are laid on the tops of transverse sleepers like a steam railroad; there is less dust and mud to contend with. Between Frankfort and Offenbach on the other hand, the rails are laid through populated streets, whereby all the disadvantages of city traffic are brought into play. The gearing of the cars on this line is very noisy and it wears out quickly. The train of wheels consists of a pinion on the motor shaft having 17 teeth, which gears into a spur wheel of 56 teeth keyed upon a countershaft. On this countershaft is a pinion of 26 teeth and this drives a spur wheel of 52 teeth, fastened to the car axle. The velocity ratio, therefore, is as 1 to 6.6, between the motor spindle and the driven axle. The armature makes about 550 revolutions when the car travels at the rate of seven and a half miles an hour. The pinions are made of hard gun metal, and they wear out in about a month. I have seen a number of rejected pinions the teeth of which were ground into the shape of a fine wedge. The spur wheels are of cast steel and they last about ten months when in continuous service. I was surprised to see these; and what with the noise, the excessive weight and other drawbacks, it made me more than ever prejudiced against such a mode of mechanical transmission on street cars. My views on this point have, however, been somewhat modified since I have been in this country. I had the privilege of being invited to assist Mr. W. Wharton, Jr., of Philadelphia, in fitting up an electric street car. There we used a set of spur gearing which was supplied by the Sprague Company. In this case the motor shaft, which runs at from 500 to 600 revolutions a minute, carries two steel pinions, one at each end of the shaft, and these pinions gear direct into two cast-iron spur wheels upon the driving axle, without any intermediate wheels. The motor is partly suspended on the axle and partly on the body of the car. We have run several hundred miles with this car under all conditions of load, gradients, and weather, on some of the roughest tracks in Philadelphia, but up to this moment not a single hitch has occurred. The gearing is practically noiseless, it is efficient and strong, and I see comparatively little indication of wear. The teeth are beautifully shaped, therefore somewhat expensive, but if they last long enough, and if they do not rattle when partially worn, this expense can be ignored. This car contains, besides its usual weight, about 3,200 pounds of storage batteries; it has to mount grades of 5 per cent., and run on curves of 35 and even 25 feet radius, which means very hard work.

FRICTION GEAR.

There is something quite fascinating in friction gear; it offers the advantages of simplicity, noiselessness, and theoretically a high efficiency; but as regards certainty of action and durability, it occupies a low position in the scale of appliances for the transmission of power. The late Professor Fleeming Jenkin devised an apparatus a few years ago, which he called "nest gearing," and of which great things were expected, and indeed it was regarded as the most perfect frictional gear ever invented.

Professor Jenkin arranged several rollers within a ring; these rollers were in contact with a central friction wheel which was attached to the motor, and the outer ring was connected to the driven object. It is well-known that in a pair of friction wheels when running together, the contact surfaces are exceedingly small, the pressure must be great, and consequently there is a rapid destruction of the said contact surfaces. Now, by placing a number of rollers within a ring, not only are the contacts multiplied, but there is a perfectly balanced pressure upon all the working parts. This kind of gearing, made of the best materials and of the finest workmanship, has been applied to the telpher line at Glynde. Its efficiency was tested and found to be somewhere near 95 per cent. In the case of telpher lines the motor and its attachments are well out of reach, suspended on tall posts, therefore dust and dirt can be absolutely excluded. The energy transmitted between the electric motor and the driving wheels averaged only 1.5 horse-power, and yet with all these advantages and the great care exercised in its construction and use, this gearing refused to act after a comparatively short space of time. The experiments were watched with intense interest by all who made mechanical science their profession, and especially by those who were seeking effective means of transmitting motion from electric motors. After a number of exhaustive tests, apparent improvements and modifications, the inventors of this nest gearing pronounced it a failure, and finally resorted to chains and cog-wheels. This fact, established after prolonged and most persistant efforts on the part of a number of the smartest mechanicians, should be a warning to all the advocates of friction gear for street car propulsion.

WORM GEARING.

Dissatisfied with the results of my early experiments, I resolved to give worm gearing a trial. I collected all the data I could find in text-books, and from the experience of other people. All the different authorities agreed on one point, and that was the efficiency, or rather the want of efficiency in worm gearing. According to various statements the *loss* with this mode of transmission was put down at from 33 to 75 per cent. This seemed to me a terrible waste of power; I was then, as now, engaged in the problem of utilizing storage batteries for street car propulsion. Storage batteries were at that time in disrepute generally, and as for putting these cumbersome things upon a car. the very idea of it was ridiculed by conservative minded men, Certain would-be experts published their views on this subject and they endeavored to prove mathematically that the battery could scarcely hold sufficient energy to propel its own weight, and that more than 50 per cent. of the power given by the charging dynamos was lost in the process of transformation. If they had known at the time that I proposed to introduce another energy-consuming apparatus in the form of worm gearing they would have declared me a lunatic. There were, however, quite a number of extenuating circumstances which encouraged me to pursue this question. I knew, from experience, that the storage battery, if properly constructed and used, could return more than 70 per cent. of the energy expended upon it; I knew also that a certain type of cell could propel several times its own weight at a speed greater than that of a horse-car.

With regard to worm gearing, I found that this method of transmission was used for hoists and for actuating machine tools, and that in some cases it has been working for twenty years without requiring any expensive repairs. During all my researches I failed to discover any reliable data on the construction and use of a worm which had such a steep thread that the worm-wheel could cause it to revolve. I did not come across any publication with figures to show the efficiency of gearing where the worm-shaft revolved at a very high velocity whilst transmitting considerable power. Just at that time the valuable researches of Professors Thurston and Kimball and Mr. Beauchamp Tower, on the subject of friction and lubrication, were published, and these revealed the fact that all the long-established laws on friction were entirely wrong. The experiments of these scientists proves conclusively that the coefficient of friction varied with the velocity, and that it decreased up to certain limits, with an increase of speed between lubricated surfaces.

Furthermore, it has been demonstrated that the coefficient of friction between the lubricated bearings is not .1 or .2, as so frequently stated in text-books, but .003 and less, and that even with moderately bad lubrication it amounted to only .01. All these and other considerations encouraged me in the belief that there is a fruitful field for further investigation, and I designed a set of worm gear of the following dimensions: Steel worm, turned and polished, 6 inches in diameter, 6 inches long, treble thread of 2 inches pitch; worm wheel of phosphor-bronze, trimmed teeth, $15\frac{9}{32}$ inches diameter, $3\frac{1}{2}$ inch face, 24 teeth. Ratio of gear, 8 to 1. The worm was fitted to the spindle of an electric motor, and the worm wheel attached to the axle of a street car, which was supported on a frame with bearings to keep the wheels off the ground. When running empty, with the worm out of gear, the motor took 6 amperes of current, and when put in gear, but without any other load, 7.75 amperes were used to drive the apparatus at the same speed. Prony brakes were then applied to both the motor shaft and the car axle, and the power was measured on each at a given E. M. F., current, and speed. The efficiency of the gearing was calculated by dividing the horse-power given off on the axle by that measured on the worm shaft, and we found that the useful effect increased with the speed, viz.: at 300 revolutions of the worm it gave 80 per cent; at 530 revolutions, 81.1 per cent; at ·676 revolutions, 85.1 per cent., and finally at 770 revolutions, 87 per cent. These results were then compared with the electrical energy supplied to the motor in each experiment and they were found to correspond very closely, after the usual electrical losses in the motor itself were deducted. The mechanical horse-power was thus checked by the electrical horse-power, and at last I applied the well-known formula for the efficiency of worm gear, which is given in every book on mechanical engineering.

$$\xi = \frac{\cot\theta \qquad 1-\mu\frac{p}{2\pi r}}{\cot(\theta-\phi) \qquad 1+\mu\frac{2\pi r}{p}}$$

Where θ is the angle of the worm thread,
μ the coefficient of friction,
r the radius of the worm, and
ϕ the angle of repose between metal and metal
ξ gives the efficiency of the pair

If the coefficient of friction is taken at .015, then the efficiency must be 87.5 per cent.

By means of the above tests and the formula we can find the coefficient of friction for the various speeds.

When this apparatus was attached to the car, I found that it required less current to propel it than it did previously with the chain gear, therefore I have adopted worm gear ever since. Mr. Holroyd Smith tried it recently on his cars, as already stated, and he told the meeting of the Society of Arts, in April last, that he "found it the most effective mode of transmitting the power from the motor spindle to the axle, taking all points into consideration."

With these remarks closes a short and incomplete chapter on
are other kinds of gearing which I have not
cannot do better than to leave the subject in
nbers present, who, I hope, will supplement,

criticise and amend the observations herein set forth, and thereby add to the general knowledge of one of the most important topics of the day. We are continually advancing; every hour marks a step forward in the direction of success. The grand general principles of the electrical transmission of energy are now well understood, but there are yet many other ways of adding to the sum of human knowledge, as for example in following up the details of construction, every one of which will add in some measure to the final triumph. Even by relating our mistakes and failures we serve a good purpose. "There are," said Professor Sylvanus Thompson, "half-forgotten observations, unconnected, unexplained, and often exceptional in character, every one of which has a significance of its own—residual phenomena, every one of which is, to the scientific man, of a value beyond price. He who investigates them patiently may find the clue to their meaning and reap a rich reward from them. They are the finger-posts in the high road of discovery."

DISCUSSION.

THE PRESIDENT:—Gentlemen, the valuable paper to which you have listened is now open for discussion. It is natural under the circumstances that as we have Mr. Reckenzaun with us but once, and as he is about leaving for Europe, that we should, so to speak, put him in the witness box, and puzzle, perplex and annoy him with questions. In that way I have no doubt we would greatly add to our stock of information on the subject. But I know Mr. Reckenzaun would, if anything, prefer a discussion rather than a series of interrogations, and I think that on the matter presented before us to-night, and with half the gentlemen in attendance directly interested in street railways as run by electricity, the subject should not lack ventilation, and I venture to believe that, should any gentleman desire to discuss some of these points with Mr. Reckenzaun, he will find a foeman worthy of his steel.

THE SECRETARY: There is one point in connection with the worm gearing which I have heard referred to before, and I presume, as Mr. Reckenzaun's name was mentioned in connection with it, he can give us further details, and that is the matter of lubrication. I understood when this question was up before, that in his use of worm gearing the worm was run in oil, and if so, I would be pleased to hear some further details.

Mr. Reckenzaun (making a sketch showing the worm and the worm wheel enclosed in a case):—The thrust bearings are generally made of several collars. There is a tin box completely around the gearing, having a hole for the shaft, with a very tight stuffing box. The whole gearing is thus closed almost air-tight. If you place ordinary lubricators above, just to start the thing, and if you place two or three inches of oil at the bottom of this tank, you will find that there is a perfect, continuous, automatic lubrication. The teeth of the worm wheel will lift up just sufficient oil to lubricate the worm, and there will be sufficient oil always running around—flying about inside the case—to keep the whole thing well lubricated. There is hardly any waste of oil going on, through leakage at the shaft, or here at these points. Thus you have a perfect lubrication that cannot fail at all. I believe this is the only question that Mr. Pope asked with reference to the lubrication. Of course you can use any lubricant you wish. Some lubricants are far more efficient than others. Naturally we should use the most efficient lubricant we can get in the market. With regard to the friction surface of the worm wheel, I have mentioned that that is made of phosphor-bronze and the worm is made of steel. These two metals have a very low coefficient of friction between them. The worm is, moreover, turned out of a solid piece of steel and perfectly polished. The phosphor-bronze wheel is a toothed gear, the teeth thoroughly trimmed, and also polished inside. They have, therefore, a perfect wearing surface to start with. The worm gearing works for a considerable length of time without any appreciable wear on the teeth at all. I think a perfectly constructed worm gear ought to run twenty years without renewal if properly lubricated. The prejudices against the worm gearing will soon wear away. Up to the present time it has been constructed on anything but scientific principles. The teeth were at a very small angle. The material used for the worm was frequently cast-iron. The worm wheel was sometimes made also of cast-iron. Sometimes they made wrought-iron worms and cast-iron wheels.

Mr. Joseph Wetzler: I would like to ask Mr. Reckenzaun if he has ever tried to use a worm which, instead of being cut cylindrically, has been cut radially so as to conform to the radius of the wheel into which it geared; that is the Hindley worm of the hour-glass form. This form is said to be more efficient than the straight-worm.

Mr. Reckenzaun: All the worm gearing I have constructed has been made on the plan of cutting it out cylindrically.

Mr. C. O. Mailloux: I would like to add by way of information, that this form of worm was the very first that was used. If I mistake not, it was the celebrated mechanician or engineer Hindley, who first devised the worm, and it was in that form that he gave it to the world — the hour-glass form as he called it. The shape of the teeth is varied as they go toward the end, near which they point inwardly in a radial form, as Mr. Wetzler has said.

With regard to the worm, I have watched with interest the evolution, so to speak, of means of transmission from the motor axle to the car axle, and I am very well prepared to agree with our friend Mr. Reckenzaun, that it is undoubtedly one of the most important problems that we have to deal with today. In fact, I may say that it is the only remaining unknown quantity in the problem of electrical traction. The problem of designing and constructing electric motors has advanced by the assistance of workers, mathematicians and others, until it is today a question of engineering, you might say, almost purely; but the manner in which we should dispose of the energy in order to render it available for moving the car, is not, unfortunately, so well taken care of; and the very fact that we see so many diversified attempts to meet the severe requirements which Mr. Reckenzaun has so well placed before us, shows that a satisfactory solution — at least one that is satisfactory to everybody — has not yet been reached. Some are very much predisposed in favor of belting, others in favor of gearing, friction appliances, etc. Now there is going to be, I believe a certain amount of competition between different forms of gearing, leaving aside for the present, other methods, such as belting, etc. There is to be some divergence of opinion for some time to come, as to which is the best form of gearing. Experiments have been made by very able persons on the subject, and so far, nearly all of them concur is ascribing to the worm a much lower efficiency than to the spur form of gearing. I have in mind some experiments which were made under the auspices of Messrs. Sellers, of Philadelphia, in which they sought to go thoroughly to the bottom of the problem, and to arrive at some conclusion as to the relative efficiencies of the various forms of gearing. The results attained showed first that the efficiency did increase with the speed, as Mr. Reckenzaun has stated, but that this efficiency did not increase always with the speed; that it reached a certain maximum just as you would expect; and a very curious circumstance was that the forms giving the highest efficiencies were those two ordinary direct spur gearings in which the shafts are parallel with each other. Starting from that they made experiments in which the angle of the two shafts was gradually increased so that there was, so to speak, a gradual transition from the form in which the two shafts were parallel, to that in which the two shafts were at right angles to each other, as in the worm gear. We may say, in fact, that worm gearing and spur gearing will be the two extremes of a certain scale of transition, intermediate between the two extremes being placed the so-called helical gearing in which the teeth are placed at an angle in spirals. The results obtained by the Messrs. Sellers, or the persons who carried on the experiments for them, showed that the nearer they approached the form of the worm, the lower was the efficiency. I have not in mind the figures of the efficiencies, but the experimenters themselves rather left us in doubt as to the value of their results, by referring to very many circumstances which, in their opinion, would vitiate some of the results obtained, and there were certain conditions of the operation of their apparatus, etc., which they were not exactly satisfied with. I must say for myself that I have always looked upon the worm with more or less suspicion. While I was not prejudiced against it, particularly after seeing the beautiful results obtained with it by Mr. Reckenzaun, in England, yet at the same time I felt that it was to be taken under probation, so to speak. There are some disadvantages, I think, which are connected with the use of the worm, and one of them which occurs to my mind just now is the difficulty in running down hill when the worm is fastened to the motor shaft. When an electrically propelled car runs down a grade it is usually found possible to shut off the current in a great many instances, and this, of course, results in a certain saving of energy. Now, unless the pitch of the thread was properly calculated, it seems to me that it would be somewhat difficult for a car to move down hill with ease, because of the well-known difficulty of moving such an arrangement backward. It is very well-known that a thread can be used to turn a toothed wheel, but it is very hard to turn the thread by means of the toothed wheel. I understand that Mr. Reckenzaun has met this difficulty, though I do not know whether he still considers it one of the things to be remedied, or whether he deems the remedy that he has proposed sufficient.

Now, turning back to the other form of transition, namely, the pinion, which I have said is likely to be one of the opponents or rivals of the worm system, I have in mind some form of that kind which I saw in use a year or so ago, which it seemed to me was very well adapted for the purpose that it had to serve. Recognizing the fact, of course, of a great tendency to wear, in the pinion or in the gear wheel, and also the great tendency to the production of noise, a special form of pinion had been devised. This was made by taking a number of thicknesses of raw-hide or leather—I think it was raw-hide—and closely packing them and threading them, so to speak, on the shaft, with nuts to hold them in place, then cutting inside a thread so as to make a pinion of these thicknesses of raw-hide. The gear-wheel, I understand, was of phosphor-bronze—it had that appearance. I made some experiments with that system at Providence, at the shops of the Rhode Island Locomotive Works. I think the name of the system was the Bentley-Knight. I found the working of it to be satisfactory. It was entirely noiseless, and the only doubt that there was in my mind as to its success was the question of durability of the pinion. I have no doubt that if there is any wear, it must necessarily come on the pinion, that being the softer substance of the two; but I have no data by which I could arrive at the comparative durability of the raw-hide pinion.

With regard to the chain, I may add a word which might be interesting by way of information. My experience with it goes to show that the loss from its use is not nearly so great as might at first be supposed. In that respect the pinion is placed in very nearly the same position that the worm gearing is. In other words, it depends largely on who is using it, and how it is used. Mr. Reckenzaun has obtained excellent results—nobody will deny that—from that worm gearing which has always been looked upon as the bane of the mechanic. Now, the same may be stated with regard to the chain belt. It is capable of giving excellent results, but in order to obtain them a special form of chain must be used. and the secret of the success which Mr. Van Depoele has achieved in this country is due largely to the special form of chain which he has used. The connecting piece between the links has a thimble around it which acts as a roller, so that instead of having a frictional action on a toothed wheel it has more of a rolling action, and by taking care to keep the chain properly lubricated, not only is the noise greatly reduced, but the loss from friction is also very largely diminished.

MR. RECKENZAUN:—I think there is only one point which requires observation on my part, and that is in regard to running down hill with worm gearing. I make the angle of this thread so that the car runs down hill very easily if we once start the worm. You can with your hands turn the worm wheel around almost at any speed you like, and I know from experience that a car which is provided with worm gearing will run down an incline of one in seventy to one in a hundred—it depends on the roughness of the track—very easily. If it were not for that, if the pitch or the angle of the worm thread were not sufficiently great there would be more danger, even in the case of running on the level road, than running down hill, because as soon as you cut off the current from the motor the momentum of the car would strip off all the teeth of the wheel. If you make that angle sufficiently large, you will find there is no trouble at all with reference to that point which Mr. Mailloux has pointed out. If you take the formula which I have given on the blackboard, and which I suppose will be published some day, and if you take the dimensions I have put down, you will find the exact angle of the worm thread, and you will find that its angle of repose is so low that the worm wheel is bound to revolve with the greatest of ease.

As regards spur gearing, I have pointed out in the course of my observations in the paper, that I have been very well satisfied, in fact, astonished with the ease with which that spur gear which we use in Philadelphia is working.

As regards chain gearing, I have been rather unfortunate with chains, but if I had employed such appliances as Mr. Mailloux or Mr. Van Depoele, no doubt I might have been more successful. Of course, Mr. Van Depoele uses rollers, and we know that rolling friction is much easier than what I call grinding friction, where the links of the chain have to grind over the teeth.

I agree with Mr. Mailloux that there will be great competition between the different forms of gearing, and it is only that form of gearing which lasts longest and gives the least trouble which will be eventually adopted generally. But at the present time we are still in the dark as to the best form. Everyone believes his own the best, because he has had the greatest experience with his own form. Any discussion of the description we have had to-night will greatly advance the subject.

MR. S. S. WHEELER:—I want to ask Mr. Reckenzaun how the gear was connected to the motor. If the motor was mounted directly on the worm shaft, did not the jarring of the car shake the motor up a good deal, and prevent the brushes resting on the commutator? I do not think he has explained that.

Mr. Reckenzaun:—No, sir, I have not explained that. The worm and its coupling are generally forged in one piece. I employ a slightly flexible coupling between the motor shaft and the worm shaft, and between the bearings and springs, which take off the jarring from the bed of the motor. There is very little jarring going on—comparatively little—and we have never had any trouble with the jumping off of the brushes from the commutator. If the springs on the commutator brushes are sufficiently strong there will be no trouble. In some cases I have tried—taking the brushes of the commutator to be almost horizontal—I have tried putting a balance on, so that the jarring, instead of jarring it off, jars it on, and puts pressure upon it. I found at last there is no necessity for that. There is very little trouble in that respect if it is properly adjusted at the outset. With this flexible coupling in the first place, and the wheels flexible to the bearings that support the frame of the motor, you will find very little trouble on the points Mr. Wheeler has mentioned.

Mr. Wheeler:—If that motor jars up and down at all it either must twist on the foundation or else there would be a fault in the shaft.

Mr. Reckenzaun:—Yes, there will be a slight fault in the shaft; but if there is a flexible coupling between, it will compensate for that. Of course we do not expect it will be half an inch out at any time, but it will allow a variation of an eighth of an inch.

Mr. Wheeler:—That ought to do it.

Mr. Mailloux:—I would like to call attention to a point which I put down as the first on my list, but which, in my anxiety to get through, I omitted. I do not wish to see it passed by, because I think it is of some importance, and perhaps throws light on these discrepancies relative to the efficiency of the worm which have been observed in the results of the best experimenters. I have reference to end-thrust in the motor. My own study of the theory and practice of the worm-wheel has convinced me that at least a large proportion of the waste of energy due to friction in worm gearing must have been due to end-thrust. Now the question arises, did not Mr. Reckenzaun, perhaps, employ means by which this end-thurst could be reduced, if not eliminated, and in that way obtain a much better efficiency than the others who had preceded him? I wanted to mention particularly that in analyzing the experiments made by Messrs. Sellers, I was not satisfied with the precautions that they seemed to have taken to eliminate that end-thrust, and perhaps that may have been the cause of considerable loss of energy by friction.

Now there is another point which occurs to me, and which I will beg the privilege to discuss while I am up. The problem of electrical traction involves a very important requirement in the matter of securing adhesion between the rails and the wheels in climbing up grades, going around curves, etc. The electric motor has not been found successful in enabling us to climb grades higher than six per cent., or at the outside, seven per cent. I have succeeded in going up a grade of six per cent. with a motor car having another car attached to it. But these are *tours de force*, so to speak, which perhaps we would not care to repeat every day. Then, if we admit the necessity for a certain amount of traction—adhesion between the motor and the rails—we arrive at the consideration of the importance of having the power transmitted to both axles. In a great many cases I have found by experience, that the transmission of the motion to one axle, was insufficient in certain kinds of weather, when the tracks were not in the right condition. There would be a very severe and annoying amount of slipping, which would greatly interfere with the progress of the car and the success of the experiment.

Now, another requirement which has presented itself in my study of the problem is this: the difficulty of starting easily, smoothly, and without hesitation, so to speak. That is a more important item of the problem than people generally imagine, and I have in mind several systems of electrical traction which are successful in almost every respect but that. They start with a certain jump which is likely to strain, if not to rupture, some part of the mechanism of transmission, and in connection with this it has suggested itself to my mind as being an eminently desirable end to attain, to have means by which the motor could be started independently of the car. As you know, there is a certain amount of energy to overcome in the motor itself, and a certain amount of energy must be expended in giving it the velocity at which it is to be run. Now, if we could do that first and not have to do it at once, or simultaneously with the overcoming of the inertia of the car itself, you will see that we will have made some gain. It would be very much like dividing a load into two portions, instead of having to lift it all at once—lifting a small part of it and then going on to lift the rest. That suggests itself to me as a very important question, and I believe that the successful system will be one in which that feature is present. I would like to hear the opinion of Mr. Reckenzaun.

Mr. Reckenzaun:—With regard to eliminating the end thrust of the motor in the case of worm gearing, I use no special precautions whatever, excepting that I employ several thrust collars. I believe Mr. John Pendleton has a system of worm gearing in which the thrust is eliminated by having two worms, one right-handed and the other a left-handed worm, so that the thrust of the one counteracts the thrust of the other. But it has this little disadvantage, that the gearing comes very near to the ground. There are two worm wheels and the motor shaft is at a tangent to the two wheels. The motor is between the two worms. By this means, the two driving wheels have to revolve in the same direction. The two worms revolve in the same direction, and being right and left-handed, the thrust is entirely eliminated. I have made one kind of worm gearing, for a special purpose, where two worms were attached to a motor which drove two worm wheels, and on the same shaft with the worm wheels were two large spur wheels, which were used for climbing up a rack. In this case, also, the thrust was eliminated, so that there was only the friction of the thread of the worm, and the worm wheel teeth.

With regard to the problem of adhesion, it is perfectly natural that the more driving wheels you have, the better will be your adhesion, because each wheel will have the same effect in assisting to start the car. I never use less than four driving wheels on my cars. I would use six if it was convenient.

Mr. Mailloux:—How do you connect them?

Mr. Reckenzaun:—I use two small trucks with pivots, on which the car body is supported. The car has two motors. There are four driving wheels and two driving axles. By extending the shaft we could have a second worm wheel; but that would complicate matters very much unless it was absolutely necessary for very steep grades.

It is nonsense to attempt to run cars on a grade of more than six per. cent. in wet weather. It is impossible. No electric car or steam car, or any self-propelled vehicle will climb grades of more than six per cent. in slippery weather with absolute certainty. If we go beyond six per cent., we must have some artificial adhesion; we must introduce some rack on the road, or cables, which have a positive motion. Any one who attempts to guarantee a self-propelled vehicle, whether propelled by steam or any other means on grades of more than seven per cent., will most certainly lose his reputation.

With reference to starting, I have always found that electric cars start with remarkable facility; that is to say, with remarkable quietness. There is very little jerk. Mr. Mailloux mentions that most electric cars start with a jerk. I know that steam cars, cable cars and horse cars start with a jerk, but I have very seldom found it on electric cars, and I have had the privilege of riding on a good many electric cars in various parts of the world. There is a gentleman here, Mr. Stein, who I think might give an opinion on that subject. He has been on our car in Philadelphia, and I think he would assure you that it starts easily. You can hardly feel the starting; it starts gradually, and that is perfectly natural. When the current is transmitted to the motor the motor has no counter-electromotive force. Therefore the whole of the current that goes through is due to the actual wire resistance of the motor. As soon as the motor is started, as soon as the armature revolves, the current decreases immediately in proportion to the relative speed of the armature. As the counter-electromotive force is created in the armature by its revolution, so the current is reduced, and it is almost impossible to start an electric car suddenly with a great jerk. I think those are the main points Mr. Mailloux touched upon.

Mr. J. M. Pendleton:—In regard to the electrical propulsion of cars, while the first efforts were being made in Paris by Mr. Philippart, in the celebrated omnibus feat, with which he paralyzed the residents of Paris by the exhibitions made on the Boulevard, and also the run from Paris to Versailles and return, my attention was drawn to the question of power contained in the accumulator for the purpose of car propulsion, and while an examination of that apparatus failed to satisfy me as to its being a mechanical arrangement affording the best efficiency, yet the results attained in that demonstration convinced me that the value of the accumulator and motor will find a better application in the replacing of horses than of anything else, and as I was devoting my attention to experiments with the accumulator during those years, I gave a good deal of study to improving the mechanical devices then adopted, which were simply the connection of a [illegible] Siemens dynamo with the countershaft, and from the countershaft to the wheels of the omnibus, which was borrowed for the purpose, and my recollection is that the apparatus weighed about [illegible]. We are all familiar with the demonstration made, and a good many are familiar with the devices used, but when I made the attempt to find something better, it appeared that according to the experience of car men, and in the opinion of those who had employed various devices with compressed air or steam, it was a very difficult subject. Our oldest car builders in conversation told me that the experiments had so far demonstrated that horses were the [illegible] things to put on cars and nothing else would do; that engines would not stand the pounding of the rail. They were very rapidly destroyed. Occasionally some invention would run smoothly for awhile, but that inevitably it came to grief in a very short period. Notwithstanding this discouragement, various devices, such as have been spoken of to-night, were reviewed one after the other, and the device which Mr. Reckenzaun has alluded to was the final [illegible] after discarding some dozen or more, and in his sketch [illegible] the principle was correctly shown. But the necessity of using the worm and worm wheel running in oil was very evident to my mind. It may be remembered by a good many here that in May, [illegible] I exhibited at the New York Electrical Society, a design for the system of car traction, having eliminated from it all the objectionable features that could be well done on paper. I exhibited it for the purpose of getting the opinion of those who were engaged with me in investigating mechanical and electrical questions. I was discouraged afterwards by the opinions of many—some of whom are here present—as to the efficiency of the worm and the worm wheel, and it is with the greatest pleasure that I hear so well defined and so thoroughly proved by practical experiments what I may say was a chance choice, more than a scientific attainment on my part. It came to me intuitively, as it were; it seemed to satisfy me better than any other. I have had the apparatus [illegible] for a good many months past, but other engagements have prevented my doing as much work in that line as I wished. The interest has increased from week to week. It is only a fortnight ago that I visited Philadelphia and had the opportunity of riding upon a car which Mr. Reckenzaun had in charge at the corner of Washington Avenue and Twenty-fourth street. I saw there a car under thorough control, mounting grades of five feet in [illegible] hundred feet easily, stopping in the centre of the grade and starting without any difficulty. The starting from a dead stop was with ease—much more readily than we would experience in a horse car; and although I had been so many years a believer and exponent of electric traction, and particularly of accumulator traction for street cars, I will state to you that I was surprised and gratified at the practical demonstration of its value that I saw. The car was run by a motor attached to only one pair of wheels. It had always been a great doubt with me whether on grades of five to the hundred, or four to the hundred, that a car would have sufficient bite upon the rail to enable it to mount. I saw the car stop on a grade, start on a grade without any slip, and go over it, and I felt that a very important question was solved by the manner in which it performed its work. Gentlemen, there is no more doubt in my mind to-day in regard to electrical propulsion by accumulators replacing horses than that I am standing here. After the demonstrations that have been made, it is only a matter of a few months when we will find one or two lines of horse-cars adopting electrical propulsion, and then you may expect it to come like the roll of the sea. There will be no supplying the demand that will arise for electrical cars. I had the pleasure that day I was in Philadelphia of being associated with two presidents of horse railways who were witnessing these experiments, and they both expressed their surprise and delight at the manner the car behaved, and at the thorough control which it was under. We have only but a brief period to wait before we will find the advantages which the adoption of a new and better system will give to the general public, and to the welfare of our cities.

The President:—There is one branch of the subject which has not been touched upon this evening, yet it merits our attention. We have so far confined the discussion to street cars, whereas this whole discussion might refer more largely to electrical cars in general. It may not be within your knowledge generally, that within the last month or two an electrical road—the largest of the kind in this country—has been put in operation in the coal mines of Pennsylvania. We have with us this evening Mr. Schlesinger, who has put that road in operation and who has it in successful operation to-day. He there handles trains of a hundred tons weight, and his experience in dealing with the particular question that was touched upon, of moving off easily and quickly, without any jarring of the machinery, would be very valuable in this connection. I think it would afford us all a good deal of pleasure to hear of his experience in the Lykens Valley coal mines.

Mr. W. M. Schlesinger:—In the Lykens Valley coal mines we enter into competition with one or two lines of horses, and also with the steam-engine. The mules were taken off entirely the second day, and I think that the steam-engine has also seen its last days in the coal mines. It is proposed wherever steam locomotives are being used to change them for electric motors in a very short period. The trains we have to handle are sometimes as large as 100 tons. We have to take them over grades of about one per cent. to two per cent and around 90 degree curves, one of them being 25 feet radius. We are obliged to start trains on these curves. The mode of transmitting the power from the motor to the wheel is chain-gearing. I found that wherever the train stands, the starting is very easy, or would be very easy if it were not for the fact that the cars are generally bumped together, and the load is not all put at once on the motor, but gradually. One car starts after the other, and this is an objectionable feature so far as the easy starting is concerned. If the whole train started as one, then it would move off very nicely and easily, without any jar at all, and that is another reason why I think it is always preferable to have the motor geared in such a manner to the car, that both start together, not the motor first, and then the car; there will always be a jar if this is not the case. It is the same thing as if the chain-gearing should have stretched. It will always be noticeable that the car starts far more heavily, and with a greater jerk, than if the chains are perfectly tight. Outside of that the weight and power required to stop the motor are so small, as compared with the power required to start the whole thing, that there is very little gained in starting one, especially the motor, before the other. The chains we are using in the Lykens Valley coal mines are the same Mr. Van Depoele is using, and side by side with that we use a chain which I have specially constructed for the purpose. The links are made of phosphor-bronze, and the pins are of steel. The chain with a thimble is not giving much satisfaction up there. I suppose it is because of the very heavy strain upon it.

Captain O. E. Michaelis:—Something that Mr. Schlesinger has just said in regard to the increased shock due to the successive movement of the cars constituting the train brings to my mind a remark that the reader of the evening also made in his paper, which I saw clearly was founded on experiments, and not simply the result of theory. That remark was that the duty upon the motor was greater in starting the train than afterwards, and Mr. Mailloux continued somewhat in the same manner. Now during the present year there was a series of remarkable experiments made at Burlington under the auspices of the Master Car Builders' Association, which will bring about, I think, an entire revolution in the method of connecting trains, and I am fain to confess that these experiments appear to upset all our preconceived theories as to the duty of the motor in starting trains. I speak on the subject because I am at present engaged on this coupler question, and therefore take some interest in it. These experiments showed that the greatest duty upon the motor, the locomotive in this case, was actually after the whole train had been started—an unexpected result. I am willing to admit that I was entirely mistaken myself, because at the very time almost of my receiving the account of these experiments, I had taken great pains to explain to a friend of mine the theory by which a train was moved by the successive picking up of the cars— a theory which was of course upset by these experiments. The result has been that the Coupler Committee of the Master Car Builders' Association—all prominent railroad men in active practice—unanimously reported at the Convention in Minneapolis that the future coupling of cars shall be tight and not loose, and have recommended the abolition of the pin and link coupler and the substitution of a close vertical hook, which I think meets the difficulties mentioned by Mr. Schlesinger.

Mr. Mailloux:—I stated that one of the difficulties was the sudden starting of the electric cars. Now I should have said that circumstances were responsible for that. It depends very much on the way in which the energy is given to the car; and when you are dealing with accumulators it is not often possible to apply the electrical current to the car in a manner sufficiently gradual and slow to prevent that sudden onslaught of motion, so to speak. That is what I had in mind in speaking of the sudden motion. If the current is supplied over circuits with devices by which it can be put on more slowly or more rapidly, according to circumstances, the case is different; but with storage batteries generally, it is far more convenient to have as few combinations; as little mechanism as possible in the regulating apparatus, so as to not complicate things. Now, with regard to the starting of the motor shaft first, the opinion I expressed was the result of experiments. I have seen the virtue and the value, of that point demonstrated quite recently by means of a system in which the motor is started first, giving free scope to the full electromotive force of the battery and allowing it to run at a very high speed, and accumulate energy in the form of momentum, so that not only that part of the work has been done but we have even stored a certain amount of energy there, which, when device is.applied, shall enable us to make use of that storage energy to start the car itself. I have seen the results demonstrated by a gentleman who has a system of traction, and I must say that it struck me very favorably indeed. He used a frictional device in the connection between the motor and the other, also combined with spur gearing; but when I saw the ease with which he started and stopped, and overcame grades, etc., I was convinced that there must be something in it. I still believe it.

Mr. Pendleton:—I called Mr. Mailloux's attention to the value of the electric motor, particularly for the purpose for which we are applying it, and that is that the electric engine has its greatest traction at the time of starting, and was therefore more particularly valuable for the purposes of car traction than any other engine we know of, and in order to get this momentum of a small armature for the purpose of moving a heavy car, it would only invariably bring about the result of a jerk, which we have none of as noticed, so far, in the actual tests and experiments made. I have failed to see it. I have noticed particularly in regard to the cable cars that it is very disagreeable, for obvious reasons, but where we are using a ten horse-power motor weighing seven hundred or eight hundred pounds, for the purpose, and the armature of which is but a fraction of the weight, the momentum there can amount to very little in a car weighing six tons, and I doubt whether any continued observation on his part—a man of such experience and intelligence—would bring about the support of his theory.

Mr. Mailloux:—I must repeat what I know to be facts. They are not mere theories or chimeras, but they are facts. Now, if you stop to reason for a little while you will readily see that when a motor is allowed to run very fast its counter electromotive force approximates very closely to its source of energy. Consequently the current through it is very small. If you put a force like a brake that tends to slacken that motor and gradually couple it with the driving axle, you will have a variable velocity ratio between the two which ratio is gradually approaching unity. As you put on a greater and greater effort you are slacking the motion of the armature and getting the benefit of its momentum, while at the same time the current is increasing through it, and in that way you have the most gradual putting on of the current that you can hope for. I have seen the experiment made so many times that I for my part do not doubt it any more.

The President:—There being no further discussion, I will simply ask Mr. Reckenzaun to make one or two closing remarks.

Mr. Reckenzaun:—I was merely going to reply to Mr. Mailloux. On the very first car which I constructed in 1882, which was worked by storage batteries, I used the momentum not only of the armature of the motor, but also the momentum of a fly-wheel, which weighed between three and four hundred weight. I put the fly-wheel upon the countershaft, and I used that gearing which I sketched on the board to-night—that peculiar gearing with the disks and a wedge shaped band. I employed the friction clutch. I thought that by starting the motor to its full speed, and with a heavy fly-wheel, that that would be a great advantage in the starting of the car, but I was greatly disappointed. The car started with a jerk, and when I did away with the fly-wheel I found that the car started with the greatest of ease, and I believe from actual experience which I have had in this direction, that it is far better to start without the momentum of the armature or the momentum of any substance connected with the armature. It is perfectly natural that an electric motor must start gradually. Take the motor off the car altogether and put it upon a table and send through it any current from any battery you like, or from any source of energy you like—fifty volts, one hundred volts, or five hundred volts—and you will find that the armature cannot start suddenly. It requires a few seconds to take up its speed, and therefore it is perfectly natural that a car propelled by electric energy must start gradually.

Our worthy president has requested me to make some concluding remarks, but the hour is late, and I believe gentlemen will be very glad to get off. I have nothing more to say. I have been exceedingly interested in the various remarks which have been made to-night, and I must own that I have learned a great deal. With regard to the general application of electricity to street car propulsion, as Mr. Pendleton has pointed out very forcibly, there is a very great future in store for us, and the time is very near when horses on street cars will be entirely abandoned. We have, I might say, almost passed the experimental stage and we are now entering the more profitable stage of manufacturing and supplying street railways with electric motors. The advantages in electric motors are not only economy, but cleanliness and great saving of space at the depots and great saving of space on the streets. If you take crowded streets, like Broadway, New York, and many other thoroughfares where every square inch of space you might save in the street is of value, if you do away with horses you save in length about twelve feet, and in width, of course the width of the track. Now, it would seem ridiculous for me to make such a remark, but if there are hundreds of cars running in a crowded street it makes many thousand square feet of space saved, and in that space other vehicles can pass. Another great advantage in electrical propulsion will be (apart from economy which is certain to be a result) that we shall be able to travel at a greater speed. Horses cannot pull a car at a greater speed than six miles an hour. The average speed of all the horse-cars in America and Europe is five miles an hour, including stoppages. Now, if we can travel at the rate of eight or ten miles an hour a great deal of time will be saved, and passengers will avail themselves more of the new mode of traction. They will save a great deal of time. The traffic is constantly increasing. I have heard recently that the street car traffic of New York alone has increased in the last ten years fifty per cent. If it increases in the next ten years fifty per cent. again it would be impossible to cope with the traffic if we employ horses at all. The elevated railroads, it appears are doing a large amount of business—almost as much as they are capable of handling, and the only loophole, it seems to me, out of the difficulty, is either by building underground railways or by propelling the street cars at a greater speed, so that the same number of cars will carry double the number of passengers in the same time. With these remarks, gentlemen, I close, and I thank you very much for your very kind attention. (Applause.)

Mr. Wheeler:—I move a vote of thanks to Mr. Reckenzaun for his interesting and instructing paper.

The motion was seconded by Mr. Wetzler, and carried.

Mr. Mailloux:—If I may be allowed to express another thought I would like to say that I wish we also had a medal, that we might be able to give it to Mr. Reckenzaun, because in the discussion of the subject that he has brought before us this evening, I, for one, and doubtless a great many others who are here—particularly those of us who are especially interested in the subject—feel that we have been dealing with the work of a master—a man who is eminent in his work and from whom we can all learn deep and valuable lessons. That is the reason, gentlemen, why I feel that we also ought to have given Mr. Reckenzaun a medal or some other substantial recognition of the valuable work that he has placed before us.

Mr. James D. Reid:—I am not able to express myself in scientific terms, but I think our good friend here has struck a strong feature of human nature, which convinces me that he has also struck the right vein in science. We all know that human life is a life of friction, that life is a struggle all the way through, even with the help of the best of food and the best of wine and the best of Scotch whisky. It is a struggle after all, and, gentlemen, I appeal to you, if, after life has done its best, the worm does not triumph in the end. (Laughter and applause.)

The meeting then adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, November, 1887. No. 2.

Special Meeting.

Held at the House of the American Society of Civil Engineers, 127 East 23d street, New York, October 11th, 1887.

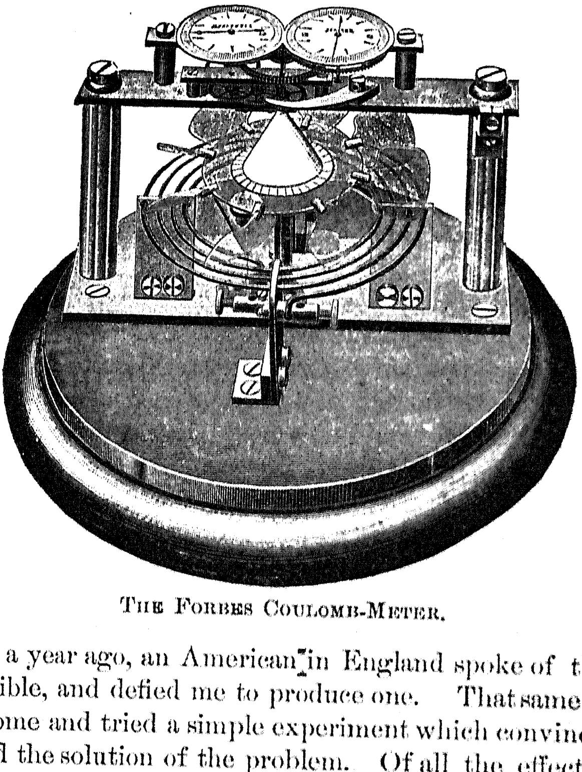
The meeting was called to order at 8 P. M. by the President of the Institute, Mr. T. C. Martin.

The President:—I need hardly say, gentlemen, that all of us have great pleasure in welcoming here this evening so distinguished a scientist and so well-known an electrician as Professor George Forbes, of England. Some of us have already had the pleasure of listening to Professor Forbes in this country, and remember the occasion of his visit here in 1884, at the time of the National Conference of Electricians and the International Electrical Exhibition in Philadelphia. Professor Forbes is also known to us through his work in England, and as the inventor of a number of highly ingenious devices, and I think by the close of the evening, you will all agree with me that in none of his inventions is greater ingenuity and grasp of electrical principles displayed than in the apparatus which he brings before us this evening. This instrument is one for which there has been a decided want, and it brings before us questions of the utmost importance and pre-eminence at the present time. I have no doubt that both the paper and the discussion, upon which we shall enter this evening, will do much to start us far along a new road of original investigation. Nothing for me remains, therefore, but to introduce to you Professor Forbes, who will now read to you his paper on a Coulomb-Meter.

A COULOMB-METER, OR INSTRUMENT FOR MEASURING THE CONSUMPTION OF ELECTRICITY.

BY PROF. GEORGE FORBES, F. R. S.

You all remember that in England, at the time when electric lighting took a decided start, there was a regular rush for illuminating towns by central station lighting. I looked into the matter at that time, and could not see that a single scheme that was then proposed was capable of being worked. We were at that time absolutely ignorant of the methods of distribution of electricity. In the first place, we were absolutely ignorant of what amount of current we might pass through wires of different sizes. One had to go all through that, and work out the problem before starting upon anything else. Then the different systems of distribution proposed had to be discussed in each particular case, to see which was the most useful, and perhaps you will remember that I myself came to the conclusion at that time that a direct supply on the multiple arc system, which was then most in vogue, could not be carried out on economical lines to a greater distance than 220 yards, and when the three wire system, the next decided advance, was introduced, it extended the distance of economical working to about a quarter of a mile. The greatest step, it seems to me, which has been taken in the distribution of electricity since the period when electric lighting took such a start in 1878, was the use of alternating currents with induction apparatus, which was simply achieved by the most indefatigable industry and plucky perseverance against the opinions of everybody who seemed to be capable of giving an opinion. I have never omitted, on any occasion when I had to speak about the distribution of electricity, to express the profound admiration I have had for the energy and pluck which was shown by Messrs. Gaulard and Gibbs in working out that system upon which so much ridicule was cast during the whole time they were making their experiments. At the present moment the only difficulty which has seemed to exist in the way of a general system of distribution, lies in the want of a meter for the purpose. It is all very well to do as we are doing just now, to charge people for lighting their houses the same price that they used to pay for gas, or to charge them at so much a lamp, and let them burn all night long or just as much as they please; that is all very well as a makeshift, but every one of us knows perfectly well that it cannot go on forever; and therefore we must have a meter for measuring the consumption of electricity in houses. With a direct or continuous current, such as has been generally used on the ordinary multiple arc on the three-wire system, the problem is not a very difficult one, and has been solved with greater or less simplicity, with greater or less accuracy, in a variety of ways. But even in that system it has always been felt that there was great room for improvement over anything which has been done. In using alternating currents, however, there has been until lately at any rate, absolutely no meter in existence which was capable of giving, even with a fair degree of accuracy, the consumption of electricity in a house.



THE FORBES COULOMB-METER.

Nearly a year ago, an American in England spoke of the thing as impossible, and defied me to produce one. That same evening I went home and tried a simple experiment which convinced me I had found the solution of the problem. Of all the effects of the electric current I was sure that the only one which would suit alternate currents was the creation of heat in a conductor. The experiment I tried was to cut a circular disk of paper into a spiral and to support this spiral at its center on a needle point. Holding this in an upward draught of air, the spiral rotates. I tested this and found that a very feeble amount of heat sufficed to produce this effect. I then put some wire under the spiral and passed an electric current through it, and found that the spiral moved with an extremely small consumption of electric energy. From this beginning, after nine months incessant work, I evolved the coulomb-meter which I now exhibit. From this work has resulted the simplest and most accurate meter conceivable, which is equally suitable for continuous or alternate currents, which is read off by indexes and dials like a common gas-meter, and which, when set in place, never needs to be touched, and readings being taken simply from the dials.

There are four dials in the latest instruments, reading units, tens, hundreds and thousands. The unit I adopt is the English Board of Trade unit, measuring the electric energy consumed in a hundred-volt lamp using one ampere of current for ten hours, the cost of which unit may be a little less than ten cents in a good system of distribution. When the inspector goes his rounds each quarter of a year, he reads off the indexes and subtracts the reading of the last quarter, and so he has directly the number of units consumed. If the lamps be not of 100 volts he must multiply his result by the volts and divide by 100 to get the number of units of electricity used up in the quarter year. I might add that the Board of Trade unit is equivalent to a thousand revolutions of the mill-wheel. This instrument is made to work up to 20 amperes, and it might work 20 hours a day for four or five months before the reading of the dial would be exhausted.

I believe that the meter which I present before you to-day is as simple in construction as any meter can be, which requires wheel-work to indicate the amount of electricity which has been used. It has wheel-work just as a gas-meter has wheel-work, so as to indicate upon dials, by means of indexes, the exact number of units of electrical energy; just as a gas-meter measures the exact number of feet of gas which has been consumed. But in making this comparison between the gas-meter and the electric-meter you must not imagine that they are to be put upon a par with each other. The gas-meter has become a proverbial instrument, not for the great accuracy with which it performs its work, but for totally different qualities from which I trust this meter before you is wholly free.

The instrument is ridiculously simple, and consists of four parts: (1) A mill-wheel (or smoke jack, as Sir Frederick Bramwell has called it); (2) a wire under the mill-wheel, which creates an upward current of air when the electric current goes through it and heats it; (3) wheel-work for counting the number of revolutions of the mill-wheel; and (4) dials and indexes for reading off the number of revolutions.

The mill-wheel consists of eight mica vanes, fixed by pieces of pith to the circumference of a circular disk of mica, at an angle of forty-five degrees to the horizon. This disk of mica has a circular hole in the middle, where it is supported by a thin paper cone, apex upward. At the top of the cone there is a jeweled cup fixed inside a pinion. The jeweled cup rests on a point fixed to the base of the instrument.

The wire which is to be heated by the electric current is of iron, and forms a flat spiral of four turns just underneath the vanes of the mill-wheel. The ends of this wire are attached to one of the wires carrying electric currents to the lamps.

The pinion on the cone of the mill-wheel works in a train of wheels, so as to reduce the speed. One thousand revolutions of the mill-wheel indicate one unit on the dials.

Each of the dials has ten divisions. The entire instrument is placed under a cover, to keep off dust and irregular currents of air.

The instrument, as now made, starts with half an ampere and is fairly accurate with three-quarters of an ampere (the amount of current generally used is an incandescent lamp.) From two lamps up to thirty lamps, the speed of revolution is exactly proportional to the current passing.

The same identical instrument will indicate from 20 to 600 lamps with the same accuracy. This is done by diverting part of the electric current from the meter by a shunt wire, so that only one-twentieth part of the whole current is measured.

If I want to measure from one lamp to six hundred lamps, I use two meters, one with and the other without the shunt. With small currents only, the meter for thirty lamps is connected. When the current for twenty lamps is passing, it is strong enough to act on an auxiliary apparatus which allows the greater part of the current to pass through the second meter. [The two meters are then in parallel.] This auxiliary apparatus is an electro-magnet with a coil of twenty turns of wire going to the first meter, and one turn going to the second. When the current is strong enough it pulls down a lever, which makes connection with the second meter and holds it there as long as it is wanted.

The want for such a meter has long been felt. Neither consumers nor suppliers of electric light and motive power can be satisfied with a price determined solely by the number of lamps in a house, and not by the hours of consumption. The meter must be simple and cheap, and must be readable by the consumer as well as by the supplier of electricity. These conditions are all fulfilled by the meter I am exhibiting to you.

In isolated plants, where storage batteries are used, this instrument enables us to see how much electricity is put into the batteries, and how much they give up.

In distributing motive power by electricity, it is quite impossible to charge the consumer by the number of motors, because the time he is actually running the motors may be very variable. A meter is indispensable.

I may now refer you to the table on the blackboard, showing the indications of the instrument, from which you will see that the ratio of speed to current is practically constant.

Current in amperes.	.25	.35	.45	.6	.75	1	2	3	6	12
Ratio $\left\{\frac{\text{current}}{\text{speed}}\right\}$ . .	.76	61.2	50.4	51	50.7	51	51	50.7	51	51.6

I cannot conclude this description without acknowledging the generous enthusiasm, free from all jealousy, with which this meter was received by English electricians when I showed it for the first time—namely, last month—at the meeting of the British Association at Manchester. I must say that the kind sympathy of these eminent men went to my heart. May I be allowed to quote the remarks of one or two from the London *Electrical Review*, September 23 ?

Sir William Thomson said, " I have very great pleasure indeed in congratulating Professor Forbes. Every one who knows anything about electric lighting understands perfectly that the want of a meter is one of the greatest practical wants in connection with the introduction of central station lighting. I speak feelingly on the subject, for I have tried year after year to produce a meter. There is nothing to which I have given up so much time in endeavoring to make an instrument work, as in endeavoring to produce an electric meter, and the Patent Office contains records of some of my attempts. Each one of them has been to some degree capable of giving a result, and the last of them, which I worked at a very great deal till four months ago, promised considerable accuracy, and was perfectly available for either direct or alternate currents. But the difficulty of seeing a way to bring this instrument out, and to make it a practical thing at a moderate cost, for general use, was so great that I do not feel that I intend to go on with it at all. It is just possible that I might make it succeed as a meter for a large station. But I am exceedingly glad to see the great success which Professor Forbes has obtained. I think I may say I am untinged with any feeling of regret at its having been done by another. On the contrary, it is with the most unmixed feelings that I say I am exceedingly glad, and that Professor Forbes is to be congratulated."

PROFESSOR PERRY said—"One has no right to ask an inventor of such a beautiful instrument as this by what means he arrived at the idea embodied in it. I could not have imagined that he could have got this result. The figures certainly show that it is a beautifully simple instrument, and effects its purpose well. But how any one could arrive at such a method from any theoretical information passes my understanding. The whole action seems to be so complicated. I do not think I should have tried it if the idea had come into my head. I suppose one must miss a tremendous lot of valuable things in this way, because one does not see the theory. It is a marvellous thing. I certainly think it is one of the most beautiful things we have had before us for a long time. I speak now as an inventor of a meter which is in considerable practical use, not in this country, unfortunately, where it is patented, but in Germany. This is also a very simple one, but I think that this beats it."

I will not trouble you with further quotations. I can only say that I was deeply touched by the sympathy of other inventors, and that the reception I met with in England led me to take this opportunity of showing the instrument to you on this side of the Atlantic.

DISCUSSION.

PROFESSOR FORBES:—I would like to show you one or two points about the meter. In this instrument there is one part which I have not described to you. Perhaps before doing so I ought to let you see it working. I will put on a battery current and we will make it go round and see what happens. I am now starting it with a current of eleven amperes.

A MEMBER:—When you break the current how soon does it stop?

PROFESSOR FORBES:—It takes a little time to get up its full speed, and a little time to lose its full speed. I have shown this lighter form of wheel-work, and I am making it even lighter than this is in the instrument that is now being manufactured. This instrument has a good deal of metal work—much more than in any of the instruments I have previously used, and the mass of metal absorbs so much heat that it goes on for a considerable time after the current is broken. Now this might suit the suppliers of electricity very well, but it would not of course suit the consumers, and one must defer to both sides. I do not want the meter to act in that way, and I find that by reducing the metal work as much as possible that difficulty is removed. There is another point about this instrument which I have not shown you or spoken about. It is an arrangement which is attached to it for lifting up the mica vanes against a fixed stop above. I have moved the stop away, you see, at present. That attachment lifts the mill-wheel up against the stop and enables us to carry the instrument about with safety, under ordinary circumstances. I must make one reservation—there is fear of danger if it passes through the New York Custom-house. (Laughter.) I brought in a lot of instruments, and after I saw a little of what the Custom-house people were doing, it sickened my heart so much that I let them lock the instruments up and put them in the public stores. I had worked six hours that morning like a slave doing their bidding, and by the time they came to me I am sorry to say that many of the things were in a sad state. You will notice something hanging to that instrument. That was an extremely delicate spiral spring, which was attached to one of these mill-wheels, acting simply as an ampere meter, not acting for registering the consumption, but measuring the rate at which the current is passing through the conductor. That instrument is quite disabled, and others, too.

I have also some diagrams here. Here are actual examples of some of the readings of these ampere-meters, and I have sometimes taken them under different conditions. I have, for example, the instrument with the clock-work in, and without the clock work, showing the amount of friction; and I may tell you this, that it looks as if the wheel-work there would cause a considerable amount of friction, which has to be overcome. That friction is a mere nothing compared with the friction of the air which is circulating in these convection currents which are created. There are three frictions coming in: First, there is the air friction in the currents of air, which is the chief source of friction; secondly, there is the friction of the jeweled cup on the point; and, lastly, there is the friction of the wheel-work, which is far less than the friction of the jeweled cup upon the point. That is what one of these diagrams shows. Another one happens to show the effect when the apparatus for fixing the mill-wheel is taken out. Another one may be of some interest to you. It was this instrument when it was in the experimental stage. I varied the distance of the vanes from the conductors, and I used it at $\frac{1}{16}$, $\frac{3}{16}$, $\frac{5}{16}$ and $\frac{7}{16}$ of an inch. You might expect that the nearer it was to the conductor the greater the velocity would be of the given current. That is not so. It then creates friction by pulling the air from the surface of the wire when it is too near. Three-sixteenths of an inch was found to be the most suitable distance. There is one peculiarity of this instrument which I believe no other meter has. I have not only my own tests of it, but I have also some independent tests. I have never been able to get any independent tests of any meter in the world; but I left word before I left England a fortnight ago to have one of these meters finished and sent to Sir William Thomson, who was kind enough to say that he would test the instrument. The sister meter to this one was sent. This meter has an adjustment to it, which no manufactured article ought to have and no manufactured meter will have; but this being in the experimental stage, it had an adjustment which regulates the depth at which the first wheel works into the principal pinion. Now it appears that that screw had become loosened in traveling, before Sir William received it, and this fact was not ascertained until after the experiments were made; but the operation is now being repeated. I have received the tests made under these adverse conditions, which I will write down on the board. He applied different currents and he obtained the ratio of current to speed at these different velocities. He did not try to ascertain the feeblest current with which it would start. He began with one ampere and the ratio of current to speed was 66.5; with 1.5 amperes the ratio was 45.5. I will leave out the decimal place if you please. He then used two amperes and the ratio of current to speed was 44. He then used three amperes and the ratio was 39. He then used five amperes and the ratio was 39. He then used ten amperes and the ratio was 42½. He then used 15 amperes and the ratio was 42. Although I look at that as very bad indeed, I think you will agree with me that it gives you a very good result. I would have been very well satisfied with that six or eight months ago; but I confess I am not satisfied with it at present, when we are able to get it so much more accurately. However, I thought it would be worth while to give you these results as obtained by Sir William Thomson. After the instrument had been running (it was one of these instruments with a large mass of metal in it) Sir William Thomson found what I have spoken to you about, that putting through the lower currents after it had been running for a long time with 15 amperes, it was too favorable to the supplier of electricity and not sufficiently favorable to the consumer. After that he put it on to 10 amperes and he got only 37 at the ratio of speed to current, instead of 42½, as it was originally.

I think that is all I have to say about the meter. I have no doubt that in the progress of discussion there may be points of interest raised, which may give me an opportunity to say something more about it, but I think I have really touched upon all the chief points of interest connected with it. (Applause.)

Prof. Forbes then passed a current through the instrument, and a recess of ten minutes was taken to enable those present to examine it. When the session was resumed, the President said:

As we have now had an opportunity to examine the instrument, the discussion will be in order, and the better ventilation this subject has, the more profitable will be the evening. We have with us this evening some few gentlemen who are not members of the Institute, and I beg to assure them that any questions they may desire to ask, and any information that they can afford us, will be heartily welcomed.

Dr. Vander Weyde:—Is it the intention to have that apparatus under the glass shade continuously while in operation?

Prof. Forbes:—At the present moment I intend to use that glass shade, although I have no great preference for it. If any one has any objection to it, a metal cover is just as good, only one would need a glass window to read it by.

DR. VANDER WEYDE:—The objection would be that when the apparatus is in operation a little while, the air will be heated and the ascending force would diminish and become very small, at least, when the air was considerably heated in the glass shade. To correct that, nothing would be easier than to make an opening in the top of the glass shade to let the hot air out and keep the surrounding atmosphere more uniform. The apparatus would give an indication of less velocity if the air is made warm, than if it is cooler. When the apparatus has been in operation for some time, so that the air which surrounds it becomes heated, the indications would become a great deal less. I wish to do justice to Professor Forbes for the ingenuity of the apparatus, and the remark I made is only for the purpose of suggesting something which may make the apparatus more perfect.

PROF. FORBES:—These are very suggestive remarks that come from Dr. Vander Weyde, and of course they occurred to me during the process of the experiments. There is one lesson which I have learned from this instrument, which is that you cannot calculate on what is going to happen with a convection current. I have been as sure as possible, over and over again, when I was making a minute change in the instrument, that I knew as well as possible what it was going to do—and it never did it. Dr. Vander Weyde suggests that when the air at the top of the shade gets heated, the instrument will move slower. The only answer I can give to it is that it does not. It goes perfectly, continuously, however many hours I have it at work. He suggests that a hole should be made at the top to keep it cool. Nothing in the world will ever induce me to let a hole get into that mechanism, any more than I would let the works of my watch be exposed to the open air to permit dust to get into them. But it is quite unnecessary, for it goes any number of hours at a perfectly constant rate, even when the top gets hot. It is remarkable that the first person who has spoken at this discussion has raised the first objection which every person who has ever spoken to me about it has raised, and that is, that its indications will depend upon the external temperature; that when the external temperature is high it will go quicker. This is the only one thing, however, in which practice does agree with theory in regard to convection currents. The theory of the thing is perfectly simple, The moving force is the difference of temperature between the conductor and the external air. It does not depend in the least on the absolute temperature of the place; it is only on the heat that is being generated in the conductor, and it is the difference between the temperature created there and the external air which gives you the moving force. It is a matter of indifference whether you start with a temperature of 30 or 100. As a matter of fact the indications in a warm atmosphere and a cold atmosphere are identical. I may forestall the question that is going to be asked immediately—whether the barometer has any effect. I may say that I have not had the instrument under an air-pump to test it, but I have tested it day after day and I have seen no perceptible variations.

Dr. Vander Weyde:—No person of any information would talk about a barometer in connection with it; but what becomes of the heat which is continually generated?

Professor Forbes:—It is radiated from the surface of the glass. A great deal collects at the top. A fixed temperature is arrived at, of course, and the heat which is being created is all radiated from the surface of the glass.

Dr. Vander Weyde:—It must be confessed however, that heat is generated continually. Now, if experience proves that that does not affect the instrument, that is all right; that settles the matter; only the suggestion is very natural.

Prof. Forbes:—Oh, certainly, most natural.

Dr. Vander Weyde:—Because we all know that all the ascending force or air depends upon the difference of temperature between the air which is pressing under the vanes and the air at the sides; and that the heat accumulating in the top must gradually descend and cause an influence, I cannot help believing.

Prof. Forbes:—You are able to test it when you are passing a high current through it; you are able to test it by holding the hand on it.

Dr. Vander Weyde:—Putting the hand on it is not a good test. We have thermometers which can determine it correctly, and I cannot see why a small hole in the top would do harm. The gentleman said he would as soon expose the works of his watch to the air as to have a hole in the top of that shade. But I would like to be informed what injury a small opening in the top would be, to let out the hot air which must come up from below.

Mr. Frank L. Pope:—The amount of heat which is generated by the action of the current is not very great at the most, and as it gradually collects in the top of the glass, it is probable that before the hot air extends down far enough to affect the operation of the instrument, the radiation has become sufficient to carry away an equal amount of heat with that which is being generated, so that the temperature from that on is stationary; and that the balance is brought about before the currents of air are affected sufficiently to interfere with the operation of the instrument.

DR. VANDER WEYDE:—But I suggested that the radiation through glass to the exterior air is very small.

MR. C. O. MAILLOUX:—If it is asked what the objection would be to a small hole there, it seems to me that it is proper to ask what good it would be, When Professor Forbes has told us that the instrument works absolutely perfect without the hole, I do not think it necessary to consider the absence of a hole there.

There are some other points which I would like to bring forward, and which, perhaps, may be of greater importance than a small hole in the top of that glass shade. One is the friction of the apparatus. I would like to know whether the calibration of the apparatus does not depend for its constancy on the friction of the wheel-work remaining constant as nearly as possible, and it also occurs to me to suggest that the friction of the apparatus may have a great deal to do with its sensitiveness as an integrating instrument. In other words, if there is very little friction, it is quite likely that the inertia would be greater, and its momentum once obtained would tend to carry it over, so that if there were any abrupt changes in the current, it would not so readily respond to them. If, then, we were to draw a curve of variations of current, it would not in reality follow that curve closely, but only partially, introducing a quantity in the result which would be the analogue of self-induction with an ordinary apparatus. Now there is another point. We all know that in order to obtain heat from a conductor we must have a certain amount of resistance, and I would like to know whether the amount of resistance that has to be introduced in the wire (which I presume is iron from its appearance) is of material consequence—whether it would figure at all as an element of resistance of the whole circuit, and also whether, by virtue of its resistance or of its position, it would introduce any variation, which has been called "impedence," in the circuit in consequence of self-induction or anything like that.

Prof. Forbes:—The first question is, as to the effect of the constancy of the friction—whether the speed of the instrument depends upon the constancy of the friction of the solid parts; and the second question I had better take in connection with it, which is, whether that friction of the solid moving parts renders it less sensitive. First, we have to consider whether, after the lapse of months or years—when the mechanical parts of the instrument are not in such good order as when it was started—its readings will be as accurate as they were at the beginning, and in the second place, will it be so ready to start with a feeble current with the friction of the wheel-work as without it. I have already said that the most important part of the friction in that apparatus is the air friction; but that is especially so in the case of a larger current. When the currents come to the average size or greater, the air friction is, out of all comparison, the greatest friction that we have in the instrument. At any time the friction of the wheel-work, incredible as it seems to most people at first sight, who are not accustomed to thinking much of the subject—the friction of the wheel-work is but a fraction of the friction of the jeweled cup upon the pivot. You must remember that the first wheel of the wheel-work is rotated at only one-tenth of the speed of the mill-wheel, and its weight is extremely light compared with the weight of the mill-wheel, and the friction of the first wheel is therefore very slight indeed, and when you come to the second wheel, its speed of rotation is diminished tenfold, and its lightness is small compared with the mill-wheel. I was told to expect this by a great friend of mine, whose name I ought always to mention in connection with this instrument—Mr. Stroh, who assisted Sir Charles Wheatstone in all of his celebrated inventions, and who has assisted me in all this work, and there is no man capable of giving such assistance in the mechanical details of an instrument like this. I had intended to make a German pun—a thing I never did in my life before—and I should have done so if it were not for the earnest objections of Mr. Stroh, and call this a *Stroh*-meter. (Applause.) That is the way that the friction of the apparatus comes in. It is very slight indeed compared with the air friction, except with very feeble currents indeed. The result, then, has always been that what I call the ratio of speed to current was constant, whether the friction was a little more or a little less, but that it did not start quite so easily with extra friction; and I have no doubt that if the wheel-work were to get seriously out of order, and we were to have very much more friction, it would not be so sensitive to a single lamp, but at higher speeds there would not be the slightest difference in the indications. For example, to show you how that works, I passed around two diagrams, I think, giving you the ratio of current to speed when different currents were passing through, with the clock-work attached, and without the clock-work attached. You will notice in those that, with the clock-work attached, it did not start with so small a current, but when the instrument was once in working order from one and a-half amperes or so, or upwards, the two lines of ratio of speed to current were identical. As to the resistance of the instrument, I am asked whether it would offer a sensible amount compared with the resistance of the lamps. As soon as it does that, we must of course stop using it. I have never been able to raise this wire to such a temperature that it did not indicate proportionately. I have had my wires red hot, and up to that point it was still indicating proportionately. You must simply stop using it when the resistance here compared with the lamps is something you do not like. There is another question asked which raises a subject that I ought to have spoken more fully of in my opening remarks. Mr. Mailloux has spoken about Mr. Oliver Heaviside's im*pe*dence. [Mr. Mailloux had pronounced the word *im*pedence.] I beg Mr. Oliver Heaviside's pardon. He has distinctly asked us not to speak of it that way, but to call it im*pe*dence. He asks me whether the effects of self-induction are perceptible in the instrument. Now there are two effects of self-induction in a coil like that; one is the effect of induction of one turn of a spiral upon another, and the other is concentration of the current in all parts of the wire, not allowing all the current to pass through the whole section of the wire equally. These are the result of using an interrupted current or any variable current. Both these effects come into play. In this case I have only four turns of the spiral, and their mutual influence is of course quite insensible. Many of you I presume read, with interest equal to my own, the delightful researches of Professor Hughes on self-induction, a year and a half ago, which he gave to the Society of Telegraph Engineers and Electricians in London. You will find recognized there the fact that a variable current passing through a conductor does not pass equally through the different sections, and it has been more generally recognized since that time than previously, that there was a greater resistance in a given wire to a variable current than to a steady current. Professor Hughes undoubtedly showed that this change of resistance is far greater in iron than in other metals. The metal I use for my conductor is iron. I admit that, having these experiments in mind and having followed out Professor Hughes's experiment myself in the laboratory at a considerable distance, I did expect to find a change in the resistance when I used an alternating current; and I expected that to use the same instrument for continuous and alternating currents would necessitate the employment of a constant by which the readings should be multiplied in order to suit it for the alternating current. I confess with the utmost gratification that on testing it by means of the Siemens dynamometer on the alternating current, and using the same Siemens dynamometer in testing it for the continuous current, there was absolutely no difference in the readings of the instrument so long as the current remained constant, whether the currents were alternating or continuous. Then I remembered that all Professor Hughes's experiments, and all those I had made in continuation of them, were with a made and broken current, in which the rapidity of change from no current to the maximum of current is terrific, and that very rapidity of change is what produces the spurious resistance—that apparent greater resistance when the broken current is being used; and that when we are using an alternating current which alternates very rapidly, it is true, but in a variable manner, in a manner whose variations are not commensurate with the rapidity of the variations in the interrupted or broken current, then those effects which were so startling in Professor Hughes's experiments do not appear sensibly. At any rate, as a matter of fact, with alternating currents and continuous currents the instrument reads identically. No constant requires to be introduced.

Mr. S. S. Wheeler: I would like to ask what is the resistance of the coil used?

Prof. Forbes: Nearly a tenth of an ohm.

Mr. Wheeler: And I would also like to ask if you cannot overcome the effect of induction by making one coil reverse upon another?

Prof. Forbes: Perfectly; but there is no effect of induction.

Mr. Geo. D'Infreville: The instrument being so sensitive, is it not desirable to take some special precaution to prevent it from registering when no current passes through it; for instance, put it in some place where it will not be subjected to a current of air, or the radiation from the sun, or the effect of hot air?

Prof. Forbes: No, I cannot say that it is. When it is under a shade, as far as I remember, I have no recollection of ever seeing the wheel moving when there was no electric current passing through it. I am pretty sure it never does move; but you must remember it requires to go round a thousand times before it registers a sixpence worth; so even if there were a few turns it would not matter a bit.

Mr. Henry Greer: I would ask if this has ever been introduced into an Edison current?

Prof. Forbes: What do you call an Edison current? (Laughter and applause.)

Mr. Greer: I mean the current that is working in that system bounded by Wall street, Nassau street, Beekman street and —(Laughter.)

Prof. Forbes: This is the first time the instrument has ever been at work in America.

Mr. Mailloux: I want to say that there is one point in the questions which I asked, that Professor Forbes did not fully answer, though he has since nearly answered it in reply to my friend Mr. D'Infreville. It is in reference to the sensitiveness of the machine as an integrating instrument. In other words, if you take the rates of variation of a current that is not varying at too rapid a rate, and then a current which we will say is comparatively steady, but which is suffering some periodical variations, somewhat like this—(illustrating on the blackboard)—if we draw another curve in which the rate of speed of the mill-wheel is used to represent corresponding rates of current, then evidently when we come here (indicating) if that has more or less inertia, then we will find that the increase of speed of the mill-wheel will be a little more, and then we will find that it will dally a little behind, and that here also (indicating) it will be a little slower. In other words, we would find that perhaps these waves would not be followed so closely. That is what I meant by impedence and the factor which would be the analogue, though not the exact equivalent, of self-induction.

Prof. Forbes:—The momentum of the apparatus has no effect. The momentum is extremely trifling. As soon as the momentum tries to carry it around at a different speed from that which is determined by the velocity of convection currents, these vanes are pulling every way through the air, and act as a drag on it, and it immediately suits itself to the velocity of the air currents. Since that question has been raised I would like to say this, that you must not expect the instrument to go jumping about at a very rapid rate with every change of lamps. With a small current it takes about five minutes to settle down to its readings. With the large currents it takes about a minute. So that really, if these were the variations in the current (illustrating), in all probability the reading you would get would perhaps not reach its maximum quite so soon, and here again it would have a curve whose variations would not be quite so great.

Mr. Mailloux:—That is exactly what I mean.

Prof. Forbes:—That is not due in the least to any momentum in the instrument. That is due to its taking a sensible time to accumulate heat in the wire and a sensible time to get rid of the heat accumulated in the wire.

Mr. Sidney F. Shelbourne:—The Professor said it took about five minutes to get its rate of motion in the mill-wheel with a feeble current. Now, in regard to the first question that was asked by Professor Vander Weyde in respect to making a hole in the top, does it not appear that the apparent inaccuracy of the instrument in registering light currents, and in getting its rate, may be due to the generation of heat at the commencement, and that heat accumulating in the top of the instrument and heating the glass, so as to arrive at a point where there is an equation between the generation of heat by the wire and the radiation from the outside of the glass? I simply suggest this, and I ask the Professor whether he ever has tried in making instruments, instead of making the whole of the cover of glass, to put a dome of copper or some other conducting metal upon it, so that the air which rises and accumulates at the top may sooner reach that equation of radiation from the outside of the instrument with the generation of the heat which operates the mill-wheel?

Prof. Forbes:—I am asked as to the inaccuracies of the instrument with a feeble current, and the cause of it. I confess that, as a user of the meter, I have not the slightest objection to its not indicating at its full speed for the first five minutes, when you are using very feeble currents. The monetary value of such a thing is imperceptible. If we were using the instrument to determine the true value of the ohm or any of those profound researches, it would be right to go to very great accuracy. But I do not think it is necessary to go to any greater accuracy than we have at present on that point, and to make the feeble currents begin their accurate reading a little sooner. I really do not think that practically it is very important. Otherwise, I think the suggestion that was made might very likely be good—using a metal shade. Of course I have had numerous changes in shades. I have tried a shade which was nearly all metal. But I found this shade before you the best one I have used up to the present time. However, I think it is quite likely that the suggestion just made might be a good one—that a metal shade would be fully as good as the other —might be better—might make it a little more sensitive, perhaps. I think there may be a good deal in what you say.

Mr. Joseph Wetzler:—I have no doubt that the instrument in its present shape has gone through sufficient experimentation, as to make any suggestion regarding its history a little out of place. But I am a little interested in knowing whether any experiments have been made to determine the best angle of those vanes and the best shape of them, and if the shape determined on there has been found to be the best.

Prof. Forbes:—As to the angle, I have committed the grossest folly that a man with the experience I have had with this meter could commit—I have trusted theory. I have been positively certain that an angle of 45 degrees must be the best. I am sorry Mr. Wetzler has drawn this confession from me, because it is really the only respect in which I have trusted the theory. As to the other points, the shape of the vanes has been the source of endless experiments. I made an enormous number of changes before I arrived at this shape, and even little details in the shape have come from experiment. I made hundreds of vanes in the experimental stage, and I am pretty sure that if that type of mill-wheel is to be used the vanes are the right shape.

Mr. Elias E. Ries:—Professor Forbes mentioned at the outset that his attention was drawn to the devising of this meter by the fact that no meter existed for measuring alternating currents. Now, during the elaborate discussions we have listened to this evening, nothing was said about its application to alternating currents. Many would like to hear from Professor Forbes as to its use in that connection, and if the meter is specially adapted to alternating currents where other meters heretofore devised are not applicable to that purpose.

PROF. FORBES:—I should be very pleased to answer any such inquiries, where there are points which I have not touched upon sufficiently. Meters in the past have been constructed by the use of, I think, only two principles of electrical action; the one being electro-chemical action and the other electro-magnetic action. Several instruments have been devised with electro-magnetic action as their basis; but those which were sensitive enough to be practical necessarily contained iron in their construction. I do not know of any meter which has been brought into practical form, which does not depend on one or the other of these two principles—the electro-chemical or the electro-magnetic principle. With an alternating current, the current is, of course, alternately going in opposite directions, and in any electro-chemical arrangement which may be used, whatever is done by the current when it is going in one direction, is undone by the next alternation of the current. So that it seems to me hopeless to think of designing a meter based upon electro-chemical action. The designing of a meter based on electro-magnetic action is far more hopeful; though even in continuous current meters I believe the difficulties have not been altogether gotten over; but in using alternating currents the phenomena of self-induction come in, and not only that, but also the fact of your having iron. The intensity of the magnetization of the iron depends upon the speed of the alternations; so that, as a matter of fact, it has been found in every meter which has iron in its construction, that the indications of the meter, even if you keep the current perfectly constant, are totally different when the speed of the engine varies; that is to say, when the number of alternations per second is changed. So that, if you were testing such a meter, you would test it at one speed and you would get at its constant—if it had a constant—and then you would give it to your consumers, and of course you would not run your engines at that speed; you would run at a totally different speed, which would favor you as suppliers. I think it is immoral to suggest such a thing as that; it lends itself to fraud. That is the objection to the electro-magnetic meters, that they give totally different indications even with the same current, when the speed of alternations is changed.

Mr. F. L. Pope:—Like a gas-meter when you change the pressure.

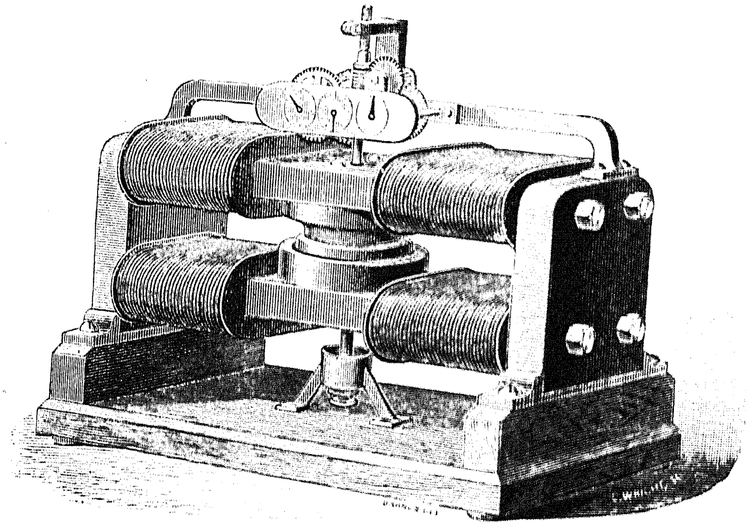
Prof. Forbes:—Yes; like a gas-meter when you change the pressure. It was thinking of these things, and thinking also of other effects of the electrical current, that made me conclude that the only thing which would do was heat. I have often thought of the question of meters before, and although your president has been kind enough to say that I have invented one or two things, I always try not to invent, if possible, because that is not my business. My profession is consulting work. But sometimes it is irresistible, and when a man defies you to do a thing which you know you can do, it is totally irresistible. I had been thinking over the matter a good deal before, but I never had made up my mind how I was going to do it; but so soon as this defiance was put before me I immediately hit upon this method. The reason, then, why this is suitable to alternating currents is, that it measures the heat generated; it measures the energy which is used up; it measures a certain fraction of the energy which is being used up in the lamps, and that is what we want to measure, and it will do so whether we are using alternating currents or continuous currents.

Mr. Ries:—The speed of the alternations per second has no effect?

Prof. Forbes:—Has no effect upon this. This simply measures the energy—a certain fraction of the energy of the lamp.

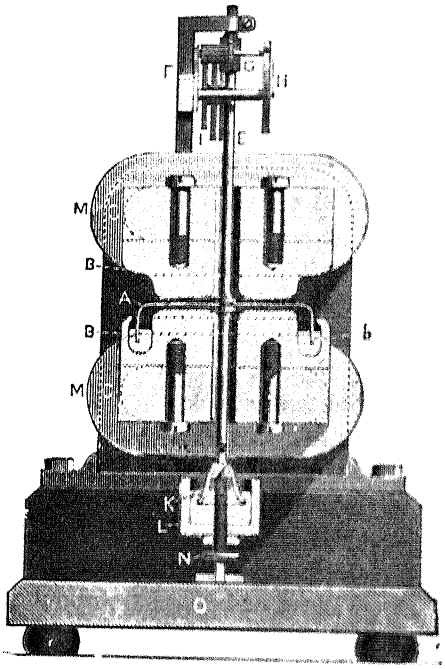
The President:—We have with us this evening a gentleman who has done more than anyone else to elevate the standard of electrical instruments of precision in this country. I allude to Mr. Edward Weston. The subject before us has already developed many points of great interest, and I have not the least doubt that Mr. Weston can contribute very largely to our information on these points, and also touch upon others that have not yet been brought forward. Mr. Weston, we should be very happy indeed to hear from you.

Mr. Edward Weston:—I do not know that I can say very much more than has already been said in regard to the instrument by Professor Forbes. I think it is an extremely ingenious and beautiful instrument. There has been already a great deal of time and a large amount of money spent on electric meters for direct currents, and I can say with Professor Forbes, that I do not think we have had any current meter of any value whatever. The electro-chemical system, in use by a very prominent company here, was put out with a great deal of confidence in the results to be obtained from it. I think anyone looking at the principles involved would condemn it from the start. You are to take a small fraction of the total current, and a very small fraction indeed, and pass it through an electrolytic cell, the diameter of which varies from time to time, and the resistance of which is never fixed, and pass the bulk of the current around a resistance which is practically fixed; the change in the resistance of the metal would be but a small proportion of the change in the resistance of the electrolyte. Now in such an instance as that, you are measuring a very small fraction of the current passing, and whatever error you have, you are multiplying that error by that fraction. If you are measuring 1-1000, you are multiplying your errors by 1,000. Anyone who has had much to do with electro-metallurgy knows that the cells are constantly changing; that to keep the deposit regular and uniform; to keep resistance of the solution uniform, they must be constantly attended to. There is not an electroplater in the country who does not once in a while go in and stir up his baths. Such an instrument is of no real value. We may, therefore, I



THE WESTON METER. (Fig. 1.)

think, dismiss those instruments entirely from consideration. A great deal of work has already been done on magnetic instruments; that is, instruments that depend upon the magnetic attraction of the current through a wire—or the current through a wire going around an iron coil. In those instruments we have all the defects which have been referred to as being possible to exist in this instrument here to-night, manifested a thousand times more, with numerous others that do not exist in this instrument at all. I can speak feelingly on the matter because I have given a great deal of attention to it, and have thought once or twice I had something very good, and have been finally obliged to abandon it. If any one will take the records of the Patent Office on electric meters, he will see there has been no small amount of time and thought, and money, spent on that question. Practically, we are in the same position as we were before. We had nothing except the electro-chemical meter, which, as I say, I consider entirely worthless. Instruments have been built which are approximations, but nothing in comparison with this, and if they were, they would not be of as universal application as this instrument is. This instrument is applicable to either alternating or direct currents, with the same practical degree of accuracy a thing very much to be desired, particularly in the present state of art. I will refer a little to some instruments that have been built in my own laboratory, just to give you an idea of the ingenuity and skill displayed in working out this instrument of Prof. Forbes's, and how nicely it overcomes all the difficulties. [The speaker here made a sketch of Weston meter, Fig. 2.] We have here an ordinary magnet with the so-called consequent poles, which in this case are in the form of disks, instead of being curved out for the insertion of an armature, These poles are brought as close together as they possibly can be. There is an annular chamber running around the lower pole, in which mercury is poured. A mercury cup is also placed on the base of the instrument, in the end of which is inserted a cork float to get as little weight on the pivot which enters the stem as possible. This stem passes up right through, connected with the disk at the center and then engages a wheel work similar to that which Professor Forbes has here for the purpose of registering the number of revolutions of the disk. Now, if this magnet, which was wound with very fine wire, was placed and so proportioned as to be saturated with the maximum current that would be likely to flow with the difference of potential on the circuit, *i. e.*, if the core was kept constantly saturated, then a current passing in would go through the disk readily, down through the cup and out through the frame of the magnet, the magnets being placed in derived circuits so as to keep them constantly in the point of saturation. Under those circumstances, of course, we should have a series of radial currents flowing out from the center to the periphery, or vice versa. Such being the case, the disk would commence to rotate and record the number of revolutions on the little wheel-work or registering apparatus employed with it. This instrument was the result of a great deal of hard work. It has been absolutely abandoned. I could not find anything that was really satisfactory, and I came to this last of all. It stands on my laboratory table now. It has not been touched for two or three years. This instrument, however, within certain limits, was really admirable, but those limits was so narrow as to entirely prohibit its use for practical work. When the speed varied, the element of friction in the mercury came into play here, and very seriously changed the rate of revolution of the instrument. I do not know whether Professor Forbes has



THE WESTON METER. (Fig. 2).

done any work involving the friction between a mass of metal revolving in mercury or not.

Prof. Forbes: I did a good deal of that at one time.

Mr. Weston: If you did, you will understand the difficulty. Nothing was accomplished. Now when I come to see an instrument of this kind, after the amount of work that has been put on mine, it makes me ashamed to think that I spent so much time on a thing of that kind. I may say that it has already given rise to a very neat form of current instrument in which, instead of having this disk rotate, it was fixed, and a large index brought out from the disk, in which case it was absolutely proportional to the number of lamps in circuit. For a station ammeter I do not know of anything that equals it to-day. It shows one lamp when there are 300, or 400, or 500 on, just as it shows one lamp by itself. Professor Forbes has got here the simplest instrument that I think could be built. I do not see how you could improve it very much except in detail. The principles are correct, and I must congratulate him very highly indeed upon the success of the instrument. (Applause.)

Mr. Ries: After Doctor Vander Weyde's suggestion, which was the first one made this evening, about that hole in the roof, it occurred to me that perhaps another principle besides those mentioned by Professor Forbes might be utilized in the construction of a meter of this class. Professor Forbes mentioned electro-chemical action, electro-magnetic action, and caloric, or heat, as the basis for a measuring instrument of this kind. Now if we could obviate the friction due to the revolution of these vanes in the air and place them in a vacuum like Crookes' radiometer, and instead of utilizing the principle of heat, use that of light to give motion to the vanes, I do not know but that would make a very delicate apparatus. It suggests a different direction in which to work. You might, perhaps, take these vanes and place them in a dark cell, surround them with a strip of platinum wire or carbon filament, and use the variations in light to change the number of revolutions per second, and that could be recorded by an apparatus similar to that used by Professor Forbes. I simply offer that as a suggestion.

Mr. Weston: I did not speak of the general class of meters. There are such meters as are dependent on the action of clockwork. A good deal of work has been done on those. I think Mr. Gardner took out one of the first patents. In this case I think the clock-work was to keep the gear-wheels moving at uniform rates, and the device for transmitting that through the recording apparatus was to enable a change to be made in the rate of recording. Of course, there has been a great variety of instruments of that class designed. Then there has been a class designed which has contemplated the quantity of gas generated by electrolysis. That can be used; but the same objection holds in that that holds in the other one, that you cannot put that instrument in the direct circuit. It must be in the derived circuit, and you must then measure the gas, which induces a great many errors which have to be compensated for. Then there have been other instruments in which it has been proposed to use strips of paper running under a stylus which could be moved more or less by a magnetic current. But you can see, when you sift them all down that there is not one which is applicable to any system of work that is now in use. This instrument appears to me to be so applicable. Some people may say that is a very delicate instrument. Well, there is a great deal of very delicate work about electric lighting, and the sooner we get men who are delicate enough to handle it the better we shall be off. You cannot wait for men to be educated or depend upon men entirely. You have got to run on with the art and the men must grow up with it. A few years ago there were no ammeters. Volt-meters used to be ridiculed. Everything was rule of thumb. People are beginning gradually to see that it is necessary to use instruments. They find it on the balance-sheet. I would just as soon think about running a manufacturing establishment where electrical apparatus is made, without delicate measuring apparatus, as I would think of running a machine shop without delicate measuring apparatus. It is just as foolish to try to get along with a foot-rule as your standard in a shop, and without using standard gauges. The man who works to 1-4000″, which should be the absolute limit on all first class work, is the man you want to employ. He will save you money, because he will do your work the same way every time, and there will be no tinkering and pottering to get things together. (Applause.)

The President:—It is not at all our practice to call upon gentlemen to speak; but I must say that I think I do but utter a general desire when I request Mr. Howell to favor us with a few remarks. He is working in a department which has been more than once alluded to this evening, and with which he is very well conversant.

Mr. J. W. Howell:—Mr. President, I was very much surprised to hear what Mr. Weston said. I am surprised that he does not know that there is a temperature correction in the Edison meter. The Edison meter has been in use industrially for five years, and I say without hesitation—and I am in a position to know the opinion of the practical men who are using the Edison meter—that they think a great deal more of it to-day than they did before, and the more they use it and the more they know of it, the better they like it.

There is one thing about that meter of Professor Forbes's which recommends it to me as the result of experience with instruments sent out for general use. There is one peculiarity about every American electrician—he has got to know what things are made out of, and if you do not show him he will find out for himself. Now that instrument shows itself. You can look at that and see what every part is, and you do not need to take it apart. I have sent out a great many instruments. The first thing a man does with them is to pull them apart and see what they are made of. There is no necessity for pulling that instrument apart—you can see all about it.

There is one thing I would like to ask Professor Forbes, and that is the effect of lowering the shade. I see he has a shade there very much higher than the instrument apparently needs. I would like to know if that is necessary to allow for air circulation?

Prof. Forbes:—Yes, it is more sensative with a higher shade.

Mr. Howell:—You find that that shade is about the right height?

Prof. Forbes:—That is about the limit. I have gone through dozens of shades to see what is the right size and shape, and that is the one I fixed upon.

Mr. Howell:—The impression that instrument makes upon me, and upon a good many I should think, is that there would be a difficulty in shipping it and getting the men who have to put it up and use it, to do so properly. I know that the men who will use those meters—the men who do our meter work in electric lighting—are not the men in whose hands I would like to put an instrument of that sort. How is that shipped, may I ask, Professor Forbes?

Prof. Forbes:—I packed the glass case separately when it came over. On this occasion I took off the vanes. I had three parcels for that instrument—one containing the clock-work and all the brass-work you see there; another containing the mill-wheel, and another containing the glass shade. They have traveled in England very large distances with that arrangement in action, which I pointed out before, by means of which the vanes are pushed up against a stop. The person who was going to use it had nothing to do except tightening up a screw as hard as it could go, and then it was free to work. There was absolutely nothing to be done except that, and I think I should do that in future if I replaced that glass shade with a metal cover. I have not replaced it with a metal cover, because you can see it work. I like the glass shade, but I dare say a metal shade with a window will, in practice, be the thing that will be adopted, in which case the whole thing will be perfectly safe to pack up in one piece. I shall certainly not allow the workman who uses it to touch the mill-wheel. I shall have that arrangement which is on it now for lowering the vanes on to a point.

Mr. Weston:—I take it, gentlemen, that the instrument exhibited here is not intended to be the instrument in use. We know that people do a good deal to get around meters. Gas companies will tell you that people will punch a hole through. But of course it is practicable to protect that as well as we can protect anything.

In regard to the electrolytic method by the direct current, I think I am aware, and I have been aware for a long time, that Mr. Edison has had a temperature correcting device in circuit that will work within certain limits. I still say, and I say it without any fear of being proved incorrect, that such a meter cannot work successfully and practically within the limits that it ought to work, and I think that is the universal testimony of consumers at any rate. I suppose there will always be more or less complaint by consumers about all kinds of meters, but I think they have been pretty well founded in that case, because I think the meter is incapable of doing as good work as we want a meter to do. I do not mean to say you cannot make a meter that will work.

Mr. Crocker:—I ran roughly over the amount of heat that would be generated in Prof. Forbes's instrument, and it came so high that I should like to run over the figures now and see if they are right. He says the resistance is a tenth of an ohm.

Prof. Forbes:—Under a tenth of an ohm I said.

Mr. Crocker:—Assuming it to be a tenth of an ohm, 20 amperes would give there two volts drop, and two volts and 20 amperes give us 40 watts, and 40 watts multiplied by 44.3, which is the only equivalent which I happen to remember, is about 1,760 foot pounds, and 770 foot pounds per heat unit, Fahrenheit, gives us two heat units roughly. That is, two pounds of water will be heated one degree Fahrenheit in one minute by Professor Forbes's resistance when it is working up to its full capacity of 20 amperes. Two pounds of water heated one degree, is equivalent to eight pounds of air heated one degree. I believe the specific heat of air is a quarter that of water. That is my recollection; it may be wrong. There are eight pounds of air heated one degree Fahrenheit. Now the contents of that shade there is approximately a quarter of a cubic foot. I should think it is a foot high and quarter of a square foot in cross section. Now there is one quarter of a cubic foot of air. One cubic foot of air weighs $\frac{8}{100}$ of a pound. A quarter of a cubic foot weighs $\frac{2}{100}$ of a pound—a fiftieth of a pound—the contents of that shade. Therefore, the heat produced in that shade would be fifty times as much as it would be in one pound of air. Eight times 50 is 400. Therefore we have 400 degrees per minute. It is a very high figure; I cannot believe that it is right. But we know that the capacity of the amount of air in that shade for heat is very small, and we also know that the amount produced in a tenth of an ohm by twenty amperes of current is quite large. Therefore, perhaps that figure is approximately right.

Prof. Forbes:—I think perhaps it would be better if I might be allowed to answer the question after it has been written out by the shorthand writer. It is rather difficult to follow the point where we went from water to air and the larger quantities, and to see that the figures were right. But I do not see that the quantity of air that is heated, or the quantity of water that is heated, has anything to do with the question. I would suggest that the speaker, instead of taking that line, would simply multiply it and find how much energy is used in the meter when twenty lamps are burning, and find out how much energy is used in the lamp when twenty lamps are burning, and divide one by the other and get the percentage. Settle upon the percentage more than which you are not going to use, and you have it. I should think very much over two per cent. or so, would be very undesirable to use. In some cases it might go up to three or four, possibly, but I should think, as a general rule, you would find two per cent. as much as one would care to use. The question of the number of lamps you use, simply depends on what amount of energy you care to use.

Mr. Crocker :—I did not mean the efficiency of the apparatus. I did not mean that Professor Forbes's meter used up any considerable fraction of the current. I merely meant to bring forward, as an interesting point, how much heat is produced in the meter in a given time, and to calculate it as well I was able to with the figures I happened to have in mind.

Prof. Forbes :—Then I would point out that theoretically you have to take in another point, and that is, that there is continual radiation going on, and that during constant running there is no addition to the heat contained in the instrument. It arrives at a constant condition when the heat generated in the wire is equivalent to the heat radiated from the surface of the glass From numerous experiments, both in Sir William Thomson's laboratory and also by Professor Tait, we have got at the constant of emissivity, the rate of radiation, and it is very easy to go on with the calculation a step further than done just now, and calculate what is the temperature which would be reached when the emissivity or the radiation from the surface is equal to the heat generated from the wire. The problem, looked at simply as a theoretical problem of interest, can actually be worked out by going a step further than the speaker has just stated.

Mr. Wheeler :—What is the approximate temperature of that globe after running a few minutes?

Prof. Forbes :—Running at its full, it is 120 perhaps.

Mr. Wheeler :—Then even at that low temperature it must be enough higher than the surrounding air to radiate 2½ heat units per minute, according to Mr. Crocker's figures.

The President :—While these calculations were being made as to the condition of the air in that globe, I was very forcibly reminded of the condition of the air in this room, and it made me wish for the time, not so far distant, I hope and believe, when both the civil engineers and ourselves will have a meeting hall worthy of the two societies.

The discussion being at an end, though by no means exhausted, I think nothing remains but to ask Prof. Forbes to close it with such remarks as occur to him as proper, and that being done we will adjourn to the next room, where a collation awaits us.

Prof. Forbes :—Mr. President, I really have very few remarks to make except to say that I feel that this discussion has been of great value to all of us. Very interesting points have been raised, and I have heard suggestions which have given me great interest indeed. Of course, in working at this instrument one started from theoretical notions, but one gave them up very soon and one had all sorts of resources at one's hand to vary the forms of these curves, some of which I have already shown you. I should like to show you the curves which I got when I first made this instrument. The curve ought to be simply a horizontal line. One of the curves which I first got was almost a U, and yet I had the greatest hope even when I got that. By two changes I was able to reduce the line down to a horizontal, and there were numerous other changes. In the course of working that instrument out, I learned more about convection currents than I knew there was to learn, and really one has power now to handle them as one pleases.

I have no more remarks to make, except that I thank every one here for the pleasant discussion we have had this evening. I think it has been very instructive, and I have felt throughout, the same kind, cordial sympathy that was shown in England. I did not expect to find every one ready to look upon it in the way in which Sir William Thomson did, without any sign of regret that it happened to be done by another. I thank every one here for the cordial way in which they have received this stranger. Applause.)

On motion of Mr. Pendleton, a vote of thanks was tendered to Professor Forbes, and the meeting adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, December, 1887. No. 3.

Sidney F. Shelbourne.

The special meeting announced for November 9th convened under exceedingly depressing circumstances.

Among those who visited the Electrical Exhibition at an early hour was Mr. Sidney F. Shelbourne, one of the charter members, and since May, 1886, one of the managers of the American Institute of Electrical Engineers. In order to more closely inspect the electric lighting machinery, he entered the enclosure where a Trenton engine running at high speed was driving a Mather dynamo. According to the statement of an eye-witness, Mr. Shelbourne, in passing the fly-wheel of the engine, was apparently drawn into it by reason of his clothing catching on the spokes, or possibly by his foot slipping at the edge of the wheel-pit. He was caught by the downward turn of the wheel, carried under, and thrown from the top by centrifugal force, his lifeless body striking against a steam pipe and falling to the floor. His death must have been instantaneous. After taking the necessary steps for the temporary care of his remains, the visiting electricians gradually assembled in the Managers' room, where President Pendleton, of the New York Electrical Society, after a brief announcement of the fatal accident, appointed a committee to prepare suitable resolutions as a tribute to the memory of the deceased, and the meeting was adjourned without day.

Mr. Shelbourne was fifty-one years of age, a graduate of Union and Yale colleges. Although a lawyer by profession, he was naturally scientific in his tastes, and for several years had devoted much labor and thought to electrical branches, more especially the underground problem. The corporation of which he was president, the New York Electric Lines Company, was organized expressly for the purpose of supplying subterranean conductors for the various electrical companies. The franchise secured by this company is believed to have been the first granted by the municipal authorities for conduit privileges in the streets of New York.

As an original claimant for the rights thus obtained, Mr. Shelbourne was vigilant in his efforts to secure what he believed to be the just reward for his foresight and perseverance. Against overwhelming odds, he has persistently but unsuccessfully pressed the attack against what he considered a usurpation of authority by the Commissioners of Electrical Subways.

Mr. Shelbourne has been equally persistent and faithful in his duties as a member and officer of the Institute. When interest in the society has lagged; when at times it has seemed that its vitality was ebbing away; when skeptical adherents doubted its future success; it was then that the vigorous presence of our departed friend stood forth as a conspicuous example of intelligent partisanship, determined and upright in his belief that the field of usefulness open to the Institute was second to none of its character in the world. Although there is a vacant seat in the Council, the cause for which he labored so earnestly has been in a measure upheld by his steadfast example, and those who have been encouraged by his presence, will strive the more arduously to elevate the Institute to that standard so fervently depicted by our late colleague.

SPECIAL MEETING, Adjourned from November 9th, 1887.

Held at the House of the American Society of Civil Engineers 127 East 23d Street, November 15, 1887.

The meeting was called to order at 8 P. M., by the President of the Institute, Mr. T. C. Martin.

THE PRESIDENT: You are all, no doubt, aware of the sad circumstances which attended our last meeting, of which the present is the adjournment. Before we proceed, therefore, to the regular business of the meeting, I will ask the Secretary to present the resolutions which have been adopted by the Council of the Institute, as a tribute of respect to the memory of Mr. Shelbourne. It may be within the recollection of most of you that Mr. Shelbourne was from the earliest time associated with the Institute, and that during the last two or three years he has taken an active part in its work as a Manager, giving his time and attention and legal ability freely to its service. We miss him very greatly, and as a member of the Council myself, I can only say that I have very often been greatly encouraged in my work for the Institute, by seeing that which he was at all times willing to undertake in its behalf.

The Secretary then read the following preamble and resolutions, which had been adopted at a special meeting of the Council called at 7:30 P. M.

WHEREAS in the death, by accident, of Sidney F. Shelbourne, the American Institute of Electrical Engineers has sustained a severe loss:

RESOLVED, That we, the members of the Council of the Institute, hereby record our deepest pain and grief at the sudden blow which has deprived the Institute of an active and unwearying Manager, and ourselves of an associate ever prompt to render assistance in the regular work of the Institute and to assume new duties and responsibilities in its behalf.

RESOLVED, That we specially recognize the value of Mr. Shelbourne's services in his revision, as a member of a Committee, of the Institute's rules and regulations, a work performed with the utmost care, discrimination and success.

RESOLVED, That we express hereby our sense of the many estimable characteristics of our friend and associate, exhibited in his work as a Manager of the Institute, his energy, his courtesy, and his persistence in what he deemed for the best interest of the body, and most likely to promote, through it, the welfare of the electrical profession.

RESOLVED, That these resolutions be spread on the minutes of the Council, and be printed in the Transactions of the Institute.

THE PRESIDENT: At the meeting at the Electrical Exhibition a committee was appointed by Mr. Pendleton, the President of the New York Electrical Society and the chairman of the meeting, to draw up resolutions with reference to the untimely death of Mr. Shelbourne. The Committee will now please report.

MR. HAMBLET, Chairman of the Committee, read the following resolutions and moved their adoption:

WHEREAS, The members in joint meeting of the New York Electrical Society and the American Institute of Electrical Engineers, learning of the sudden death of their associate and comrade, Sidney F. Shelbourne, at the moment of their assembly, which, being communicated by the Chairman, brought a resolution for immediate adjournment in view of the calamity, and the appointment of a committee to draft resolutions appropriate as an expression of their grief at this loss, as well as a means of conveying a tribute to the memory of the deceased:

RESOLVED, That in recording the death of our valued associate, Sidney F. Shelbourne, we desire to pay a tribute to his personal worth and good qualities, as seen both in his friendly relationship with the Society and in his services as a member and officer of the Institute. We deeply regret and deplore the calamity of his untimely and accidental death, depriving us and the electrical profession of an earnest worker, always willing, upright, genial and considerate in his connection with us.

The resolutions were unanimously adopted.

THE PRESIDENT: The resolutions will accordingly be entered in the Transactions of the Institute, and a copy will, as requested, be forwarded to the Electrical Society.

The subject before us this evening is one of the greatest interest and will be presented to us by a gentleman whom we all recognize as foremost in his profession in this country. It deals with recent improvements in apparatus for ocean telegraphy, and I have great pleasure in requesting Mr. Cuttriss to proceed with the reading of his paper.

RECENT IMPROVEMENTS IN APPARATUS FOR OCEAN CABLING.

BY CHARLES CUTTRISS.

In order that the improvements embodied in the instrument you may have seen at the American Institute Electrical Exhibition may be clearly appreciated, it would be well, perhaps, if I were to mention briefly the instruments previously used on the Atlantic and other long submarine cables.

The mirror galvanometer was undoubtedly the first commercially successful instrument ever used on an Atlantic cable, and you are all so familiar with it as a measuring instrument that a description will be superfluous, but perhaps it will be well to point out its defects as an instrument by which messages are received: Firstly, it requires two operators to work it, one to call out the signals, the other to write them down. At a first glance there would not seem to be much room for errors on the part of the *writer;* as the operator is called who writes the message letter for letter from the other's dictation; but you will see that they *can* easily be made when your attention has been drawn to the similarity in sound between "B," "P," "V," "M," "N," and other letters when rapidly called out. This similarity, of course depends largely upon the more or less clear enunciation of the speaker. The liability to misunderstand a letter on the part of the writer tells severely against the *rapidity* of the service as well as its accuracy, for this reason: supposing the writer misses a letter in a code message; he has no possible way of telling from the context what it should be; he cannot interrupt the receiver to ask him to repeat it, therefore his only course is to omit it and trust to his reader's recollection of the particular word at the end of the message. If he should not remember it, they must then delay the traffic by getting the word repeated from the distant station.

In order to avoid this liability to misunderstand one another, some operators get into the way of calling out the letter and immediately following it with some short word whose initial letter would correspond with the one called out, such as "n" for "nuts," "m" for "mutton," etc.

Another cause for delay arose from the fact that some readers would never trust to the correctness of their writer, but would always stop at the end of a message to scan it over in order, if possible, to detect any error that might have been made. While all these things added to the delay in transmission, one can hardly blame the clerks; for if an error was discovered at headquarters it meant a fine of $\frac{1}{16}$, $\frac{1}{8}$ or $\frac{1}{4}$ a day's pay, in which all three of the clerks—the transmitter, receiver and writer—had to participate. It was impossible to lay it at the door of any particular one, as each of them would most emphatically deny the soft impeachment of the possibility of *his* making an error.

These causes of delay are now obviated by the use of the recorder, as the operator can always take a second glance at any doubtful signal. Were it not for this liability to error on the part of the mirror clerks, I doubt if that instrument would have been so entirely superseded by the recorder, for its simplicity is a great recommendation. It requires very little skill in its adjustment, and with clerks that work well together, its speed is as great, if not slightly greater, than that of the old form of recorder.

During the years 1868 and 1869, while I was in Sir William Thomson's laboratory at Glasgow University, his first siphon recorder was constructed. I well remember the instrument, the electro-magnet of which was a ring of iron about 1 foot in diameter; 1 inch thick and 7 inches deep. The signal coil was rectangular in shape, about $6\frac{1}{2}$ inches long by 2 inches wide. The electro-motor for drawing the paper slip was an ordinary 5 or 7 armatured wheel with two electro-magnets. It was provided with two bands, one of which turned some wheelwork, which drew the paper slip, while the other rapidly revolved a small generator or replenisher which produced by induction, static electricity for maintaining the electrification of the ink. This replenisher worked on the same principle as those used for maintaining the potential of the Leyden jar of Sir William's quadrant electrometer. The whole arrangement looked like a medley of different instruments selected at random from a laboratory supply; yet each had its different part to perform, and it was wonderful to see the results that were obtained, when they were working harmoniously together under the direction of the masterly mind of the man who organized them, and who stands peerless either as an inventor, a scientist or patient instructor.

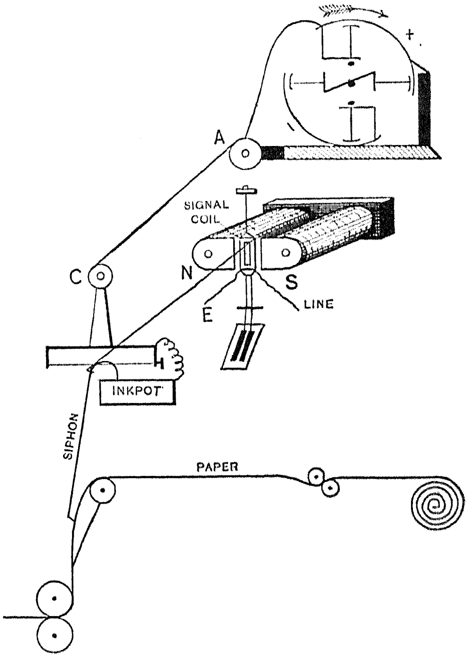


DIAGRAM OF THE THOMSON SIPHON RECORDER.

Some three years later, one of the most improved recorders was sent out to the cable station at Duxbury, Mass., and as you may not be acquainted with its construction I will describe it briefly, in order that you may more readily understand the changes that have been made in it up to the present time.

The upper part of the diagram represents the "mouse mill," as it is commonly called, which generates the static electricity that is conducted from the drum A containing a coiled spring, along the moist string and thin wire which you see stretched fom *A* to *E;* the ink pot is supported on a baked wood arm and is metallically connected with the frame to which the siphon is suspended, this in turn being connected with the milled nut *C*. *N* and *S* are the respective poles of a strong electro or permanent magnet, between which is seen an oblong piece of soft iron, the purpose of which is to concentrate the magnetic lines of force into two fields, through which the two sides of the rectangular signal coil pass. The signal coil which is connected to the cable is suspended by fine silk cord, and care is taken that it hangs perfectly clear from all the iron of the magnetic circuit. Attached to the bottom of the signal coil will be seen two strong silk fibres which pass behind a brass bar, into which a series of shallow notches are cut. To the ends of the fibres are attached lead weights which slide in grooves that are tilted slightly out of the perpendicular, the lower ends being the nearest to the observer; by this arrangement the fibres pass at a slight angle partly around the serrated brass bar, and consequently are held securely from any lateral displacement in any one of the notches in which they may be placed. These weights and fibres hold the signal coil in the magnetic field and prevent its moving in any direction other than a circular one, with the suspension cord as an axis, and any movement in this direction is resisted by the two fibres supporting the lead weights, which tend constantly to keep the coil in the centre of the magnetic fields, and therefore the movements of the coil are controlled almost entirely by these fibres.

The siphon is attached, as shown, to a brass frame, from the bent ends of which is stretched a fine brass wire kept under a constant strain by a curved spring at one end. To the other end of the spring is attached a milled nut by which the wire can be twisted so as to put a lateral strain on the siphon and its carrier. A small lug projects from this carrier and to it is attached the fibre the other end of which is fastened to one corner of the signal coil. The signal coil is now twisted slightly in a direction such as to draw the fibre, and with it the siphon, away from the observer. If now the milled head on the brass frame is turned so as to bring the end of the siphon again toward the observer, and into the centre of the paper slip which passes in front of its lower point, every motion of the signal coil must be followed by the siphon; and as the fibre is attached so near the strained brass wire the movements of the signal coil will be greatly multiplied.

The marking of the siphon is done as follows: The mouse-mill generates, static electricity, which, being conducted along the string and wire from *A* to *C* electrifies the ink contained in the ink-pot, and as the ink-pot is insulated, the only way that the electricity can escape is by means of the column of ink contained in the siphon. It does not discharge as is commonly supposed across the air space between the siphon point and the moving paper slip, but the electrified ink draws the siphon which contains it bodily toward the paper (which is slightly damp) until they touch, when the siphon is immediately discharged to earth, and by its elasticity flies back to its former position, by which time a sufficient charge of electricity has accumulated to again attract it to the paper. These movements to and from the face of the paper, which are termed the "vibration," take place at a speed determined by the natural rate of vibration of the siphon, which is generally between 80 and 100 per second; hence we can obtain a record of the potential of the cable as many as 80 times a second; and as these potentials are determined by the operator sending at the distant station they constitute the signals at the receiving stations.

The arrangement adopted in this instrument to regulate the supply of static electricity to the ink-pot was by interposing a piece of moist string between the spring drum *A* and the milled head *C*. This string had a fine wire attached to one end, so that by turning the milled head in one direction the length of the moist string interposed between the two points could be varied and likewise the electric discharge to the ink-pot.

This arrangement worked very well at Glasgow, where the atmosphere is to a great extent saturated with moisture; but in this country the string was soon dry, and then the ink would not be electrified, and consequently the record would fail. To remedy this defect I made use of the brush discharge by connecting an adjustable wire provided with a sharp point to the drum *A*, and a flat piece of brass to the milled head *C*. This was found to work perfectly, and that principle has been adopted in all electrified ink receivers made since that date. But we soon began to experience a greater trouble than the one before spoken of, and one that still exists in all recorders using static electricity as a means of vibrating the siphon; that is, the great difficulty experienced in generating a supply of static electricity during damp weather, and in keeping the arm that supports the ink-pot dry. Everything will work beautifully in nice, fine weather, but during the moist, clammy dog-days of August, and the foggy days that are liable to come on at most cable stations without a moment's warning—then it is that trouble of the most aggravating description falls upon us. Suddenly the record fails. Examination shows that the generator or supporting arm of the ink-pot has become damp. Lamps are lighted and placed near the instrument, *sometimes* with a satisfactory effect, but oftentimes not. In the latter case there is no remedy but to stop working the recorder and go over to the mirror instrument while the generator is being dismounted and thoroughly dried; but meanwhile valuable time has been lost, and business has been delayed.

Everything imaginable was tried in order to overcome this difficulty, but without success, and at last we realized that in order to make the instrument more reliable, we must find some way of obtaining a record without depending upon the electrification of the ink.

The first suggestion came from Mr. John Gott, the superintendent of the St. Pierre station, namely, to make the siphon a shade larger than usual, and put a few camel hairs in the tube at its marking point, and, as it were, paint the record on the paper slip. This worked very well, but the siphons would occasionally clog up, as we were at that time using commercial writing ink. The hairs acting as a strainer would arrest the larger particles. When you realize the smallness of the tube, the bore of which is only $\frac{1}{200}$ of an inch in diameter, the wonder is *not* that it got clogged up, but that the ink would flow through it all. The next step in advance was taken one day when the hairs accidentally came out of a siphon, the point of which happened to be particularly smooth and square with the paper; it was then found that a very good and reliable record was made by carefully adjusting the paper up to the point of the siphon until it touched it. The paper was then drawn slightly backward again; the capillary attraction being sufficient to keep the siphon from leaving it, while at the same time the friction was greatly reduced. This manner of recording has been used ever since that day on the "Dy" section of the "69" cable, and it has also been adopted on many others whose length does not exceed 1,000 miles, but they must have a conductor of low resistance.

About the year 1876, I constructed a recorder of the most simple kind, in which the signal coil was pivoted and to which the siphon was directly attached. The controlling springs or fibres were dispensed with by attaching a small piece of iron to one corner of the coil and bringing it under the influence of another movable piece of iron on one of the pole pieces of the electro-magnet. It was during the construction of this instrument that I was endeavoring to vibrate the siphon mechanically and also by means of magnetism, but at that time I did not succeed in getting a perfectly straight and steady line. As the mode of working before described was so satisfactory, I did not pursue my experiments any further, for inasmuch as I had eliminated the troubles incident to the use of electrified ink at the Duxbury station, I did not realize that they still existed at other stations where the methods we adopted could not be practiced, owing to the greater length of the cable. However, Mr. Dickenson, of Heart's Content, N. F., was not idle, and during the year 1885 he invented and patented a method of vibrating the siphon mechanically. The method practiced by him was to prolong the cradle carrying the siphon. He then attached a magnet and armature at the end of the frame carrying the strained wire, and connected the prolongation of the cradle to the armature by a silk fibre. When a battery is connected to the magnet and its circuit is rapidly interrupted, the motion of the armature is communicated to the siphon by means of the connecting fibre. What success this invention has met with in practice I am unable to state.

When I became identified with the Commercial Cable Company, I found that it was decided to prolong their ocean cable, which landed at Coney Island, into the heart of this city by means of an underground cable, and to use a recording instrument upon it. The type of cable and the high rate of speed at which it was to be worked required that the instruments should be of the most sensitive class, hence electrification of the ink or some other device for preventing friction between the siphon and the paper slip must be used. Therefore, as nothing else offered we adopted the electrified ink recorder. It was not long before the old troubles were encountered, as the office was very small, and we were obliged to keep the windows open for ventilation. Consequently it would be only a short time after a rain storm began before I heard the old cry, "the ink has failed." This was such a constant source of trouble that I determined to try again and find some other way of vibrating the siphon. After mature thought, and with the experience gained from previous failures, I was convinced that, in order to make any device work successfully and still leave the instrument as sensitive as by the use of electrified ink, the siphon must hang perfectly free and not be retarded in its lateral movements by any extraneous fibres, and that, at the instant it touched the paper to leave its dot of ink, the point must be locked, as it were, to prevent its rebounding laterally, in case the surface of the paper should be slightly irregular. The only resource seemed to be magnetism applied as nearly as possible in the same manner as had previously been practiced with electricity. I determined therefore that the part of the siphon under its influence must be the marking point. The question then arose in my mind as to the possibility of attaching a sufficiently large piece of iron to the end of the siphon without weighting it to a degree that would be detrimental to the clearness of the signals, by reason of its increased inertia. Another very doubtful point was, would it be possible to construct a magnet with a field of force so uniform that a siphon tipped with iron would vibrate at right angles to the plane of its polar face without any tendency to irregular lateral movements, or in other words, if it were placed near the edge of the magnet, would it not have a tendency to be drawn toward the centre ? When the idea was first conceived, I must admit that I had the greatest doubts of its success. But nevertheless I determined to give it a trial, so I attached a toothed wheel to the shaft of an electromotor, so as to act as a contact breaker for the local circuit, containing the electro-magnet with which it was desired to vibrate the siphon which had its point armed with a small piece of iron, and was attached to the usual strained wire suspension of a recorder, held firmly in a vice on the same table upon which the motor was running. The siphon was now approached by the rapidly vitalized electro-magnet, but without its showing any signs of vibration. Thinking that perhaps the battery was not strong enough to energize the electro-magnet sufficiently, more was added to the circuit, but still without effect. I then removed the electro-magnet, intending to replace it with a more powerful one, but the motor was still running ; so imagine my surprise the next time I looked toward the table, when I saw the siphon vibrating beautifully, describing an arc of about half an inch.

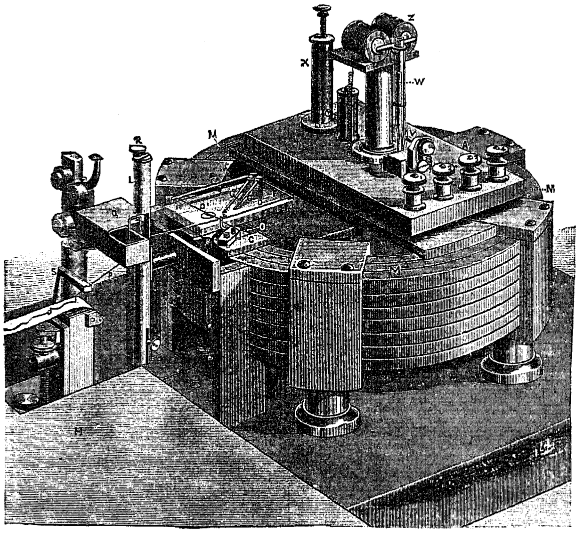
Thus without any apparent cause it was doing by itself what I had vainly endeavored to *make* it do with a powerful electromagnet. However, the solution of the mystery was soon arrived at, as I accidentally touched the table and felt a very slight but regular vibration, then everything was clear and I saw that the motor put the table into a state of vibration, the rate of which was exactly equal to that of the natural rate of the siphon; which, therefore, picked up the vibrations and showed them in a greatly magnified form. I realized plainly from this experiment that to make the siphon vibrate by magnetism all that was necessary was to vitalize the magnet at a rate equal to the natural period of the siphon. In a short time, by means of a second experiment, I succeeded in producing these vibrations by magnetism and found them just as regular at the edge as at the centre of the magnet, the siphon not having the slightest tendency to move laterally from any position in which it was placed.

It would be uninteresting to you to follow me through all the numerous experiments that were made before the vibrator was perfected. But I will mention one instance that shows how simple may be the cure for the unforseen difficulties that constantly arise when one is trying to perfect an instrument. When the first vibrator was tried, in which the mercury was used to alter its rate, it was noticed that the record would fail with great regularity for about an inch. The trouble was located in the glass tube, and was due to some of the mercury climbing up it under the influence of the vibration and then falling back to the main body. This, of course, made variations in the rate, and was fatal to the instrument unless it could be remedied. I tried putting weights on the top of the mercury, but they would not work satisfactorily, and at last, when almost in despair, the remedy was found in two drops of glycerine. The viscosity of the glycerine, I find is so great that it prevents the column of mercury breaking up.

This method of vibrating the siphon by magnetism has been applied to all the Thomson recorders in use on the Commercial Cable Company's system for upwards of a year with the greatest success. Failures of the ink are now almost unheard of, and the recorder, so far as ink failures are concerned, could, if necessary, be worked under a shed during a rainstorm or fog, as well as though it were in a warm room.

In all the Thomson recorders that I have seen, the movements of the signal coil are controlled by the lead weights before spoken of, and the clearness of definition and shape of the signals depend almost entirely upon the adjustment of the fibres which are attached to the signal coil and support these weights. In practice more or less trouble is found in maintaining such an adjustment as will give the best shaped signal. This is owing to the dust which collects upon the weights, wedging them fast in the grooves. Consequently any change of temperature, or humidity of the air, will alter the tension on one or both of the fibres, and thus oftentimes render the signals unreadable. So much trouble was experienced from this cause at our Canso station that it led the superintendent, Mr. S. S. Dickenson (who was ably seconded by Mr. J. C. Upham, the mechanician), to devise an adjustment in which the weights were discarded and a spring used in their place. The fibres could also be spread to any desired extent, and as the strain on both of them was exactly the same, the signal coil was always in a truly central position in the magnetic field.

Since this improvement has been applied to the instruments the adjustment has rarely been changed, and, what is of still greater value, a decided increase of speed has been effected. With the exception of these recorders, it is always necessary to stop work when it is desired to make any change in the adjustment of the controlling fibres, as they are very inaccessible. This, together with their occasionally breaking, led me to try and construct an instrument in which these difficulties would be removed. The result has been the instrument now on exhibition at the American Institute.



THE CUTTRISS SIPHON RECORDER.

The siphon P is a fine glass tube about one hundredth of an inch diameter, one end of which writes upon the paper slip J and has attached to its point a piece of No. 30 iron wire about one-eighth of an inch long, which serves as an armature and is controlled by the impulses of the magnetic table T over which the paper slip passes. The impulses of the magnetic table are controlled by the vibrator, which is placed on the large permanent magnets $M\,M$, and consists of a glass tube W which is connected by a tube of india-rubber to the mercury reservoir X. The glass tube is supported by a steel rod carrying a spring contact point V, and carries an armature, seen opposite the poles of the small electro-magnet Z. When a battery of three or four gravity cells is connected to the terminals 1 and 4, the glass commences to vibrate at a rate determined by the height of the column of mercury in the glass tube W, the height of which can be regulated by turning the milled thumbscrew at the top of the mercury reservoir which connects with a plunger within the latter.

From the terminals 2 and 3, two wires connect to the electro-magnet of the magnetic table T. Thus every time the contacts at V are closed the table is energized and the siphon is drawn down to the face of the paper by the attraction of the magnetic table for the small piece of iron which is attached to the point of the siphon. If the impulses given to the siphon are in accordance with its natural rate of vibration a succession of fine dots of ink will be left on the paper in a perfectly straight line so long as the siphon is not moved laterally by the coil R and a supply of ink is maintained in the ink-pot Q. Should the impulses given to the siphon by the vibrator not be in accordance with the natural rate of vibration of the siphon, it will not vibrate and the record will fail. But by manipulating the plunger in the tube X, the height of the column of mercury W is varied, so that the rate of the impulses can be maintained anywhere between 60 and 100 per second. Hence any siphon that would be suitable for the instrument can be controlled, as its rate will lie within these limits. When the proper height of the mercury has been once determined, which is generally found in less than half a minute, the siphon will continue to vibrate until it is broken or removed for other causes.

The signal coil R, which is moved by the currents transmitted from the distant station, is pivoted at the bottom and top B in agate bearings, and is free to move in the annular space between the poles of the permanent magnet M. On the top of the coil there is attached an aluminum extension E, which has a narrow slot at its upper end through which passes the regulating fibre D', one end of which is attached to the rigid support O, the other end being fastened to a flexible spring F. The strain on this spring and this fibre is regulated by moving the index G' and signal coil R. Close to the point E and at right angle to the fibre L' there is another fibre D, one end of which is attached to an exceedingly delicate spring *F*, the other end is fastened to an extension from a fine brass wire contained in the tube *L*, to which the siphon is secured. The upper end of this strained wire is attached to the thumbscrew *K*, so that by turning the latter the siphon can be moved laterally over the face of the paper to any required position.

Thus it will be seen that if the siphon is moved off the paper slip toward the front of the instrument when the fibre *D* is strained by moving the spring *F* outwardly, the siphon can be pulled by it into a position over the centre of the paper; the torsion on the strained wire and the strain on the spring, now balance each other, and if the two cross fibres are fastened together at a point as near as possible to the extension *E*, any movement of the coil will be multiplied and transmitted by means of the fibre *D* to the siphon. By lifting the fibres out of the slot *E* and turning the coil toward the front of the instrument, the coil and part of the pole piece *C* can be removed from the instrument without disturbing any other part, and, when replaced, no readjustment will be necessary.

It has been suggested to me that pivoting the coil was a step backward, as it introduced friction, and would mar the definiteness of the signals. I can only say, in reply, that when the instrument is adjusted, and giving good signals at a rate of 30 words per minute, I cannot detect the slightest friction. If I deflect the siphon laterally only the width of a hair it immediately, on its release, returns accurately to zero, which it certainly would not do if there was any friction worth speaking of in the pivots.

I have received good signals on this instrument at a speed of 30 five-letter words per minute, over our New York-Canso cable, which has a length of 980 miles, capacity of 231 microfarads and copper resistance of 13,680 ohms.

I do not intend to stop at this speed, but think that with the next instrument, in which some of the parts will be slightly modified, a speed of 25 or 26 code words a minute will be attained over our long sections, which have a length of 2,740 miles, capacity 874 microfarads and copper resistance 6,967 ohms.

I will not take up much more of your time this evening, but as the title of this paper is so broad I am loath to leave the subject without mentioning the great advance that has been made during the last few years in the working of the Morse system on cables.

Before the year 1879 no section of the Atlantic cables had been worked by any other method than mirror or recorder. That the Morse *has* been successfully introduced on two sections is due to the determination and persistence of Mr. G. G. Ward, of this city, at whose request Mr. T. J. Wilmot, now superintendent at Waterville, Ireland, applied himself day and night to the task of originating a translating device by which, in the year 1879, Torbay, in Nova Scotia, was put in direct communication with New York.

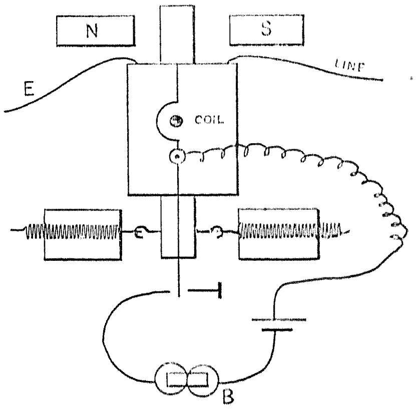


DIAGRAM OF THE ALLEN AND BROWN RELAY.

This same device, in which the Allen and Brown relay is used as a receiving instrument on the cable circuit, is now worked on our Rockport and "Cs" section. The difficulties that were surmounted will be better appreciated when I mention that the length of cable in circuit is 610 miles, with an inductive capacity of 168 microfarads, and a copper resistance of 8,482 ohms. The total circuit, inclusive of the land line, is 942 miles.

The Allen and Brown relay consists of a coil through which passes a soft iron core. The ends of the core project into the magnetic field produced by the permanent magnets *N S*, and are consequently polarized by them. In the diagram, the mark in the centre of the rectangle represents the axis of the coil. If, now, we connect the coil to the cable circuit and have two fixed contact points for the local circuit, as in an ordinary relay, the first impulse, or say the letter "S," which consists of three dots, will close the local circuit; but as the cable does not lose its charge rapidly, as is the case with a land line, the coil will not return to the back-stop and be ready to close the circuit again with the second impulse of the letter, but will be held fast to the front contact, keeping the sounder closed all the time. This, of course, will prevent any more signals being received over a cable, and some means had to be found by which the armature tongue would open the circuit whenever the potential of the cable fell ever so slightly from its maximum. This was accomplished by allowing the armature tongue, which is called the "jockey," to have a movement around an axis of its own. It was therefore pivoted, as shown, slightly in advance of the axis of the coil; you will now see that if the two points which control the local circuit are adjusted very closely together, allowing the end of the jockey a play of only $\frac{1}{1000}$ of an inch, every time that the coil falls back toward its centre position (after having been deflected by the receiving current) only $\frac{1}{1000}$ of an inch, the local circuit will be opened, and *vice versa* any increase in the potential sufficient to move the coil the same amount will again close the local circuit. By this means the coil is moved sometimes considerably in one direction, and although by watching it very closely no backward movement can be detected, the jockey will open the circuit, sometimes two or three times, proving that there have been corresponding falls in potential. On the axis of the jockey is a very delicate spring which can be pressed down by a small nut so that just sufficient friction is maintained on its jockey to enable it to close the local circuit firmly enough to pass the signals to the local circuit.

Some few changes have been made in the jockey of the relay by the superintendents at the terminal cable stations—Mr. Dickenson at "Cs" and Mr. R. Hearn at Rockport—until at last such perfection has been arrived at that the signals are passed from Canso to New York in such a perfect manner that none but an expert could tell that there was any cable in the circuit. This is, I believe, the longest section of cable on which the Morse system is worked, and, although repeated efforts have been made to attain the same results on a cable only about 100 miles longer, the retardation was so severe that the speed attained was too low to allow this system to be adopted. Gentlemen, I thank you for the patience with which you have listened to me this evening and I regret that time will not allow me to touch on several other interesting branches of investigation, such as curb sending, the duplex, automatic transmission, etc. But the object of this paper will have been attained if you realize that, although seldom heard from, there *are* many patient investigators in our remote cable stations, and that to their unflagging energy is due the fact that we are now easily able to handle a bulk of business that would have staggered us a few years ago, and do it with so little delay that a message can be sent from New York and delivered in London in less time than you can send one from down-town to Harlem.

Mr. Pendleton here took the chair and stated that Mr. Martin would read a paper contributed to the discussion by Mr. Lockwood.

Mr. Martin then read the following paper:

Supplementary Note on the Siphon Recorder and Cable Telephony.

BY THOMAS D. LOCKWOOD.

As some of you are aware, I was present at the time and place appointed for our meeting, to hear Mr. Cuttriss explain and describe the improvements he has effected in the siphon recorder.

Although the unfortunate accident which we all deplore deprived us of the pleasure of hearing Mr. Cuttriss at that time, and myself perhaps for all time, still I congratulate myself that my visit to New York has at least given me a personal knowledge which I had never before possessed, of that beautiful instrument, the siphon recorder. I have acquired a new illustration of the impossibility of obtaining an accurate knowledge of the construction and operation of a delicate piece of electrical apparatus from text-books. I had studied the siphon recorder in books, but found myself ignorant of it when the opportunity of examination at length presented itself.

It was especially gratifying to me to learn that we were to hear about an important branch of telegraphy, for I think that telegraphy has been of late neglected.

Though I yield to no man, in my admiration for the immense recent amplification of the work of electricity, or in my appreciation of motors, lighting apparatus and heavy currents, I love variety, and have found it somewhat palling to read and hear as much about them as I have had to of late; for just as the human stomach quails at the idea of eating quail more than forty meals in succession, so the human mind revolts at the notion of considering electrical currents due to an E. M. F. of from 600 to

1000 volts perpetually; and longs to demi-volt once in a while. Therefore, I hailed with joy the announcement that we were to hear about the siphon recorder.

I learned that Mr. Cuttriss's recorder was operated by a permanent magnet instead of an electro-magnet. That is an advance which appeals strongly to me; and it means, as I look at it, a considerable diminution in the amount of attendance required. When I consider the number of telephones in use now, and reflect what a number of cells of battery would have been required, had it not been discovered that permanent magnets could be utilized, and that the use of the battery was chiefly to make the magnetism, I cannot but appreciate that this is a material advance step.

Then let us look a little further; I understand from the text-books that on long cables it was found that, if the siphon end were allowed to touch the paper, the freedom of its movements was interfered with, and that it was therefore found necessary to oppositely electrify the ink and the paper, the former being thus caused to issue and to mark the paper in a succession of fine dots.

If I understand the instrument of Mr. Cuttriss, it dispenses with this feature, which must have often marred the usefulness of the recorder, since on our eastern coast the atmosphere is usually rather "hygroscopic." I can well imagine occasions when all the ability of the operators and electricians must have been called into play to get the electrifying machine—the "mousemill," I believe, to work—in places and at times when the air was surcharged with moisture, and the "relative humidity," as the Signal Service calls it, was up in the nineties per cent.

It is interesting to note with Prescott, that while the telegraph of the United States, the Morse, which made its proud boast that it was a recorder, (and by the way the first recorder,) has now almost totally been transformed into an acoustic or aural transient signal; the Cook and Wheatstone telegraphic idea, the visual semaphore, has evolved into the wonderfully delicate recorder before us.

I am also desirous of calling the attention of the Institute to the fact recorded in a letter of Mr. John Gott, dated St. Pierre, Miquelon, March 24th, 1887, to the Society of Telegraph Engineers, that the siphon recorder was capable of transmitting and reproducing speech telephonically, and that Mr. Gott had actually used it as a telephone. As I think this circumstance deserves a wider recognition, I will quote certain portions of the communication.

Mr. Gott says:—"With the object of stimulating inquiry into the means of improving the telephone of Mr. Bell, which is the most beautiful adaptation of telegraphy ever made, I desire to draw attention to a few simple methods by which any one may satisfy himself of its practicability, for no one having witnessed its performance can fail to see a great future before it. The recorder of Sir William Thomson affords a ready means of speaking, and gives out such clear tones as to make the listener at first involuntarily look for the speaker behind the instrument (who may be miles away). It suffices to take a tube two inches in diameter, and stretch over one end a membrane of parchment or thin gutta-percha (the latter is less affected by the breath, the former becoming somewhat flaccid after a time). To the centre of the membrane cement a straw, and fix the tube in front of the instrument; about six inches from the movable coil, cement the other end of the straw to the coil at the point where the silk fibre is usually fixed; this is all that is necessary for both speaking and receiving. Six or eight cells on the electro-magnets suffice.

"A pair of these tubes may also be connected in a similar manner with the tongues of the two polarized relays. The tube is to be fixed in a convenient position at right angles to the tongue, taking care that the latter is free from its ordinary contact points. No battery is required for speaking with this arrangement. Or a pair of these speaking tubes may be connected with the ordinary armatures of any instruments or relay, and a current kept on the line.

"By these simple means—may be demonstrated the possibility of speaking over many miles of telegraph line. The sound in the tube is not that of a whisper but of a voice at a distance; I have spoken through four knots of buried cable without sensible diminution of effect."

Since writing the forgoing it has been suggested to me that I might say a few words on the possibility, or possible practicability, of telephonic transmission over long submarine cables.

There is, I think, little to say on this head. You are all aware that in telegraphy it has been found possible to clear cables of charge by sending after each impulse of definite direction, a clearing impulse of opposite direction by which the static charge of the conductor was neutralized and the line cleared for the transmission of a second signal.

The first idea regarding the transmission of telephonic or voice currents over submarine cables, is in favor of its practicability; for it may be argued, since the foregoing practice aids the transmission of telegraphic signals, and since telephonic currents, as usually transmitted, consist of a series of rapid alternations, why have we not here just the conditions of success.

The truth is we have not. For in telephony the changes occurring are infinitely *too* rapid. They cannot serve the purpose of clearing the line; they rather serve the purpose of blocking the line, for the successive and consecutive impulses of opposing direction, succeed one another so swiftly that they practically neutralize one another.

Take for example the magneto telephone. It is well known that the currents generated by this instrument when used as a transmitter are alternating, and follow one another with extreme rapidity. So that for a long time it was thought that they were so infinitesimal as to be immeasureable.

Suppose we have a fine galvanometer, and telephone through it. We see no deflection, but not because the current is so small, but because the plus currents succeed the minus currents of like potential and strength so fast that the needle cannot respond, and therefore does the next best thing and stays at zero.

Let us now consider the battery transmitter and induction coil. A moment's consideration will show us that we have a close analogue to the operation of the magneto telephone.

For though it is true that in the battery transmitter the action of the diaphragm merely varies the resistance of the circuit; and consequently the *strength of current correspondently but in inverse* ratio, yet it is none the less true that the use of the induction coil, develops currents in the secondary circuit and line which are alternating, because caused by the successive rise and fall of current in the primary circuit.

Consequently these currents are subject for long circuits of high capacity to the same drawbacks as those of the magneto telephone. We have then at present only one other alternative method, namely the plan of including the battery and transmitter in the main line, and allowing the transmitter to vary a permanent current flowing in the main line, thus dispensing altogether with the coil. But this also is impracticable, because the very small proportion of variation in the total resistance of such a circuit, cannot develop fluctuations of sufficient strength to satisfactorily influence a receiving instrument.

I am therefore convinced that electric telephony—that is the transmission and reproduction of the human voice—will not, at least in the near future, be possible over long cables.

Electro-static induction and the retardation and prolongation resulting therefrom is the reason.

When I say *long* cables, I mean for example those over 100 miles long; and it would be difficult and not commercially successful over much shorter distances.

My conviction is that, those who advertise the operation of telephones over Atlantic cables, either do not know what they are talking about; or knowing, wilfully make misstatements for prospective personal gain. I am very sorry that I cannot be present to hear Mr. Cuttriss, and the discussion of the subject, but will certainly enjoy reading it afterwards.

Discussion.

The President:—Gentlemen, you have heard the paper of Mr. Cuttriss and Mr. Lockwood's somewhat informal comments thereon, and it is now open for discussion. I will take the opportunity of saying that should there be any gentlemen present who are not members of the Institute we should be very glad to hear from them should they wish to elicit any points, or in case they may be able to give us information with regard to the subject now before us.

Mr. Joseph Wetzler:—I would like to hear from Mr. Cuttriss as to what his experience has been in telephoning over cables.

Mr. Cuttriss:—My experience in trying to telephone over cables commenced, I think, the same year, or at any rate the year after the telephone was brought out by Professor Bell. I was then at Duxbury. The late Mr. Gower, whom I presume most of you know of, came down there with the best telephones made, up to that date, to try and speak over the Duxbury cable. He also brought down with him his electric harp, as he termed it, which was a series of reeds that could be struck by hammers the same as in a pianoforte, which would vibrate the contact points underneath the reeds and send of course the same number of vibrations into the line to which the reed was tuned; and it was his desire to put that on the cable and have the superintendent, Mr. Gott, at St. Pierre, listen at the telephone for any sounds. He evidently, at that early date, appreciated the fact that it would be easier to get an intermittent current over a cable than it would be to get speech over it. We listened, and Mr. Gott reported nothing. He tried varying his power to the limits to which we allowed him to departments of electrical application that we seem to have left that of cabling to our European, more particularly our British brethren, as a sort of consolation cup. And they have certainly made remarkable progress in that field and can in that respect and in that department set us an example. We have with us this evening one or two gentlemen who are quite prominently identified with some of these departments of work, and there is one gentleman here that, could he be induced, I would like very much to address the meeting. He is not altogether a stranger in this city and is certainly very well known to every one who has had anything to do with cabling. I refer to Captain Trott of the cable repairing steamer *Minia*, which happens to be in port at the present time. (Applause). Captain Trott is a very reticent man, but if we could once get him started he would give us such a wealth of information that it would take a good deal of time to digest it. Captain Trott, it will afford us extreme pleasure to hear from you this evening.

CAPTAIN TROTT:—Your President has intimated that Captain Trott is rather reticent. That is not altogether Captain Trott's fault. We belong to an old telegraph company, the oldest Atlantic company. We represent the first Atlantic Cable Company —that is the Anglo-American. I am its representative here with you, and as such it is hardly open to me to say all that I know on submarine telegraphy. The branch of it before you here to-night is not in my line of experience. It belongs to the electrical department. My special duty is to lay and repair cables. I was connected first with the Direct cable.

I was in command of the ship on the laying of that cable, and also the French cable; but now those gentlemen who were with us are in opposition to us. We are friends just the same, but I hardly think it right that we should give them all that we still have, when they are fighting us.

As you will have inferred from the description of the recorder here to-night, submarine telegraphy is rather a dry subject, except our branch of it, which is very wet sometimes. I do not know that I could interest you much. It would take too long to go into all the details of our work. They are very difficult at times and at other times we consider them tolerably easy. Of course, as you know, we have to do the fishing for these things at the bottom of the ocean, and we have played our part at that; and no matter what the gentlemen present may think of that go, but still Mr. Gott reported there was nothing. We then asked Mr. Gott at his end to send intermittent currents into the cable, through condensers and also without condensers. These were from a key. Nothing was heard; it was absolute death in the telephone. Mr Gower would not believe, nor could we convince him, that the telephone was properly connected with the cable; so to prove it I connected his telephone in circuit with the recorder and instructed the party at the other end to send regular telegraphic signals into the cable. By this means they must come through the telephone and on their way to earth also pass through the recorder. If the signals came, we must see them. The consequence was, every signal came out written perfectly on the recorder, which we all read; but still it was death in the telephone —not a symptom of a sound. Still Mr. Gower was doubtful and I did not know what to do. He did not like to doubt us, and yet we could see that he *was* doubtful. At last it struck me that I would put a rapid rheotome in the circuit with the telephone. Now, if there is any current coming over the cable at all, although it is infinitely weak when it is received at the receiving station, this rapid make-and-break would vary it so much as to certainly make it audible in a telephone. The next time the signals were transmitted over the cable, the make-and-break being started, the room was filled with the sound of the pitch of the note that the make-and-break took, showing that the cable current was all the time passing through the telephone; but its wave was so slow and so even that although the diaphragm responded to every one, it was totally beneath the capacity of any human ear to take the waves up and hear them as sound. Of course you will all readily understand that sending not probably more than 100 signals in the circuit at the time, the vibrations coming in waves of that period would not be audible, because no ear can take up sound at that rate. That has been my experience. We have tried it over our cables here in New York for several parties, and we have also conducted experiments for Professor Farmer, within the last year, over our Rockport cable, to see if we could get any sounds in that way. But in every case it is absolute death, and the greatest distance at which I have heard signals on a telephone has been through about 75 miles of artificial cable.

THE PRESIDENT:—This cable subject is one that is not comparatively new to most of us in America, but somewhat unfamiliar. In America we have made so much progress in all the other departments of electrical application that we seem to have left that of cabling to our European, more particularly our British brethren, as a sort of consolation cup. And they have certainly made remarkable progress in that field and can in that respect and in that department set us an example. We have with us this evening one or two gentlemen who are quite prominently identified with some of these departments of work, and there is one gentleman here that, could he be induced, I would like very much to address the meeting. He is not altogether a stranger in this city and is certainly very well known to every one who has had anything to do with cabling. I refer to Captain Trott of the cable repairing steamer *Minia*, which happens to be in port at the present time. (Applause). Captain Trott is a very reticent man, but if we could once get him started he would give us such a wealth of information that it would take a good deal of time to digest it. Captain Trott, it will afford us extreme pleasure to hear from you this evening.

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If any of you choose to come on board my ship which is in the dry dock at the Erie Basin, we can show you some things there that perhaps you would be interested in. We have no secrets. We are prepared to show anything that we have there. A great mystery has sometimes been made of submarine telegraphy; I think a little too much so—a feeling has prevailed that only certain individuals should know anything about it. We look upon cable telegraphy as a thing that is pretty well known to-day, and therefore we have nothing, so far as we are concerned, either as a ship, or a company, but what we are quite willing the world should look at.

I perhaps might say one word on the question of telephoning over submarine cables. My chief engineer is not here, but two of his assistants are present. They may remember. They were not with us at the time, but I dare say they know something about it. We did once put a telephone to the cable, I think 83 miles from shore, and spoke through it and heard their conversation back. I think that is the utmost which was done. We have had singing through the cable at much greater distance, and they could tell on shore the songs that were sung on the ship; but there was nothing in the shape of conversation, I think, beyond that distance, to my knowledge.

Mr. Schuyler S. Wheeler:—How deep can you grapple a cable.

Captain Trott:—To the bottom, sir; twenty-six hundred fathoms is the deepest water I have worked in.

The Secretary:—We understood from Mr. Cuttriss that we were indebted to Mr. Ward for the progress made in introducing Morse on the cables. A great proportion of American electricians know something about Morse, and of course they are interested in knowing just what has been done, and what more there is a prospect of doing, and I presume that information on that point will be welcome if Mr. Ward could give us any further details regarding it. I should for one be very glad to hear from him.

MR. GEORGE G. WARD:—Mr. Cuttriss has told you as far as we have gone. We are working Morse on the Rockport cable. It was tried on the cable between St. Pierre and Cape Cod, but we found we could get no good results from it and it was abandoned. Since then nothing has been done on it, and the best results that have been obtained are, I believe, on this Rockport cable of ours between Canso and Rockport.

A MEMBER:—Would not Mr. Cuttriss kindly tell us something of the vibrator invented by Mr. Pescod that has been in use for several years past.

MR. CUTTRISS:—In reply to that, I would state that I cannot say anything about the vibrator. I have never seen it, nor have I seen any drawing of the method. I have heard that he does it by vibrating the whole coil under the siphon of a small Thomson recorder in which the siphon is attached to the coil. He vibrates the whole thing so that he lifts the siphon a small distance off the surface of the paper. That, I believe, is what his invention is. Whether he has made any other improvement since I heard of that I, do not know. I know that there is a counter claim between he and Mr. Dickenson. As to which is right in the matter I have no knowledge, and consequently I should not want to take up any ground one way or the other. The method is the same as Mr. Dickenson's practically, with the exception that Pescod vibrates the whole coil and Dickenson merely vibrates the siphon by pulling it with the paper.

MR. WARD:—With regard to the Morse, Mr. President, I should like to say that the relay used is the invention of an American gentleman. You said that the British were taking the steam out of the Americans in the cables; but this Allen and Brown relay was invented by a Mr. Brown, formerly associated with Mr. Edison in his automatic system. Mr. Allen is the financial part of the invention. So the British will not take credit for that.

MR. WHEELER:—I think this question of telephoning through cables is so important that it ought not to be passed over lightly, and I would like to ask Mr. Cuttriss to tell us something about what can be done to overcome the induction. I mean, has the effect of a complete metallic circuit been tried, or have there been any anti-induction devices used?

MR. CUTTRISS:—Mr. President, that is a hard one. Until you can change the nature of a cable I don't think there is much hope of doing anything. Of course metallic circuits have been tried. I do not know that you gain anything by them. In fact if the two circuits are parallel from any distance I think that you probably would lose by them, for you are getting this whole reaction between the two parallel wires; that would act the same as a condenser, and there I must say that once speaking with Mr. Edison about this matter, he told me that he did not believe it would be possible for us to speak from the corner of Wall street down to the Coney Island hut through our underground cables for that very reason. He said he thought all the current would be neutralized, so to speak, or short-circuited probably would be the better term, before it got down to Water street. In that he was wrong. I have spoken from Wall street down to Coney Island very recently; and in one case where we had to open our cables to search for some trouble, at Flatbush, not half way to Coney Island, I spoke from the office to Flatbush direct, quite easily; but found that after concluding my test through that part, and having previously instructed them to go on the other end of the circuit, which was looped through our Coney Island hut and back to New York, so that we worked through three times the circuit, part of which was laid double and parallel; it was as much as I could do to hear the voices. Whether that arose from any action between the two cores, or what it was due to, I have not been able to determine, nor have I had any chances for pursuing experiments in that line. But I do not think that with long cables, a metallic circuit is going to be of any help. Of course a great deal depends upon where your circuit is. If you are in a straight away country road, then I should say, as I before stated, that I think the straight line is the better; but if you are coming over the Brooklyn Bridge, then I should say you would do better to come in a metallic circuit. Otherwise you get no speech at all on account of the induction of the other wires passing over the same route.

Mr. Wheeler:—Are we to understand from that, that a double wire would overcome the difficulty caused by the static capacity of the cable, but would bring up a greater difficulty, namely self-induction between the two limbs of the circuit.

Mr. Cuttriss:—I cannot say that I think the double circuit is going to overcome the static capacity at all. It cannot do it. The static capacity is still there. You would reduce your static capacity by using the two to about one-half. But at the same time you are doubling the resistance. Of course, with a telephone that would not amount to much, because it would go through the resistance very easily, but it would bring about somewhat this effect of short circuiting across, the same as trying to talk through two parallel plates of a condenser. Of course this is all conjecture on my part, not having had a chance to try it. I think you would gain nothing by working over a good road clear of outside induction—you would get no gain by working in metallic circuit. I think a straight away line would be as good.

THE SECRETARY:—If I understand the gentleman correctly, the capacity of the recorders as now operated is such that it does not appear that anything is to be gained by telephoning over a cable, excepting as a matter of experiment; that is, unless the telephone system was a great deal better than our local service, I would much prefer a siphon recorder for accuracy in the transmission of business.

MR. CUTTRISS:—On that point probably I could give a little light. The same day that Mr. Gower came down to Duxbury to try his electric harp he informed us what a wonderfully beautiful thing this was going to be to speak across the ocean. Well, having that trouble with the mirror, in distinguishing between the sounds "M" and "N," "P" and "B" and other letters, we rather doubted it, but we thought we would make a trial of it. So he went up stairs with one telephone, and we had the smartest of our clerks with a telephone down at the other end, and we took up what was then one of the most beautiful of cable messages, a "Stoker." If a man could make anything out of the words of a "Stoker" message he could understand almost anything. We told him to speak that message through his telephone to the clerk down stairs who should write it down, and then the clerk should repeat it back to Mr. Gower, who should also correct it from the copy. I think, if my memory serves me right, the message contained something like forty words. The clerk down stairs made over eighty errors. It was repeated back to Mr. Gower. There were two telephones in the circuit. Mr. Gower was listening at one and another gentleman at the other, and Mr. Gower corrected, as he supposed, forty of the errors; but there immediately arose a dispute between Mr. Gower and the other gentleman as to what certain letters were, that were being sent. Now, there were two people listening in the same circuit and yet they could not agree as to what one letter was. So long as a telephone will not speak more clearly than that through a room you can imagine what it will be through a long cable.

THE SECRETARY:—That coincides with my early experience with the telephone. Our telephone line was connected with the Western Union Supply Department and I had occasion to inquire of that Department about the shipment of three or four carboys of acid to Charleston, South Carolina. I got along with the whole business until the man at the other end attempted to tell me the name of the schooner by which it was shipped. It was a simple name, Mary Jane or something of that kind. But of course there was no context to give me a clue as to what the name was. In other words, to telephone successfully you must have an inkling of the business that is to be transacted, and who the person is who is sending it. I have had occasion once in a while to answer the inquiry, "Who is at the telephone?" and before I could succeed in making them understand that my name was "Pope" I would get disgusted and give it up. I presume that is one of the names that does not go through a telephone very well. This is one of the difficulties I have always apprehended in connection with this proposition of telephoning commercial messages, and those who were most ardent in their advocacy of telephoning messages for commercial purposes were those who were not familiar with the facility with which a Morse operator can receive either proper nouns or cipher messages or anything of that kind, and the same would apply to cable messages. Of course most of us understand that the bulk of the business transmitted over the cables is in cipher, and I doubt whether it would be possible to carry on the cable business as it is to-day with the best working telephone line there is in existence.

MR. F. BENNETT:—I understand Mr. Cuttriss to say that the diaphragm of the telephone responded to the undulations of the current but not in rapid enough succession to be audible. Now, supposing this diaphragm had a needle on it such as there is on the phonograph and that the cylinder like that of the phonograph, was to be rotated at a given speed, and the undulations of the diaphragm repeated on the tin foil of the phonograph, and after the message was sent, let the cylinder be rotated at a more rapid pace, would it not cause the undulations of speech to be reproduced so rapidly as to be audible to the human ear?

MR. CUTTRISS:—In my reply I would say that it certainly would not be heard at the other end. There is no reason why the current from the phonograph should be any stronger than one from any intermediate source. The electrical harp that I spoke of Mr. Gower's using, was merely a rapidly interrupted circuit, according to any note that he struck. The harp contained about two octaves, and of course we had any range, within those two octaves in which to try it, but no sound was received there at all, and in that, he used, I think, four cells of Leclanche and a powerful induction coil, the current from which was strong enough to give you a very disagreeable shock. So you can see, if that would not go, there would be no reason why anything from a phonograph would. It is exactly as Mr. Lockwood said in his paper; the currents are small and go with such infinite rapidity that they are neutralized before they get to the other end. They are actually swamped, and that is the reason why you cannot get anything through. The receiving currents of course came through his instrument because they came slow and were powerful enough to deflect his diaphragm. If we had a make-and-break on there it is possible we could have opened and broken the circuit from the diaphragm. Still, you can see why we could get no audible effect, for the moment we began to increase our speed sufficiently to bring it up to the point, we were sending the currents so rapidly into the cable that they were beginning to neutralize one another, and before we could get to the audible rate they were neutralized—probably before they got twenty miles from the station.

MR. BENNETT:—I think Mr. Cuttriss did not fully understand my meaning. We will take for illustration the old register where it makes the dots and dashes on the paper. Of course we understand that that rotates at a given velocity, the same as does the cylinder of the phonograph. Now, supposing the cylinder on the phonograph was revolving the same as it does in the siphon recorder, it would cause the undulations, or the dots and dashes for instance, to make indentations in the tin-foil, and then by revolving the cylinder faster it would cause sharper vibrations in the diaphragm and therefore cause it to be more audible.

MR. CUTTRISS:—Certainly, that would be so. But what advantage would you get from obtaining a sound in that way, if it took you probably half an hour to get sufficient signals over the cable to move the diaphragm; that is, if it took you half an hour to get sufficient signals over the cable to be made audible through the diphragm, by rotating the cylinder afterwards rapidly, I do not see what gain you would get, because your signals would have to come very slowly to indent the tin-foil at all, and you are certainly going to get no gain, because you cannot send reversals, that is alternate positive and negative currents. You cannot send the same number of words over it per minute as you can by using what we term a mixed signal. The reversal has got to be a charge to one polarity from another—a mixed signal, as we term it, the same as in the letter "S," where it goes up with the wave; all you need is that wave, and the length of the wave, to show that three signals have gone. Therefore, the sending of reversals through the cable does not increase the speed.

The Secretary:—I would say before we close, Mr. President, that these questions that have come up in regard to what can and what cannot be done over a cable, can be looked at, I presume, from two different points—from the experimental point, and from the commercial point. As matters of experiment they may be very interesting, but commercially we must remember that the cable is a very expensive affair; its service is expensive, and it is necessary to utilize every moment of time. Consequently any system that is introduced must be an improvement in speed, or an improvement by reason of doing away with these detentions and faults in apparatus which Mr. Cuttriss has brought to our attention in describing the older forms of instruments. As I understand it, the improvement has been in the continuity of the service rather than the speed, that is, doing away with detentions caused by repeating back signals, and also by failures of the apparatus.

The President:—The hour of closing having arrived and there being no further discussion the meeting is adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. V. New York, January, 1888. No. 4.

SPECIAL MEETING.

Held at the House of the American Society of Civil Engineers, 127 East 23d Street, New York, December 6th 1887.

The meeting was called to order at 8 p.m., by the President of the Institute, Mr. T. C. Martin.

THE PRESIDENT:—The subject which we have before us this evening, is one of the first importance. I therefore have much pleasure in asking Mr. William Stanley, Jr., the electrician of the Westinghouse Electric Company to read us his paper on the "Phenomena of Retardation in the Induction Coil."

PHENOMENA OF RETARDATION IN THE INDUCTION COIL.

BY WM. STANLEY, JR.

This paper will attempt to emphasize the time relations of the alternating E. M. F.'s. and currents in the induction coil. Any description of the phenomena of retardation accompanying the inductive effects of currents would necessarily be incomplete and unintelligible without a clear conception of the fundamental principles of the induction coil. This must be my excuse for briefly recalling to you the rudiments of the induction system.

An alternating E. M. F. differs from a direct E. M. F. in two important points: It varies continuously, and its direction or polar tendencies alternate.

To illustrate graphically the various values of an alternating E. M. F. and the attending phenomena, it is usual to plot a series of curves above and below a straight line. This line, when divided in equal divisions, may be taken to represent successive periods of time (See Fig. 1). Starting at the point *A*, the E. M.

F. is seen to rise rapidly above the line *A X* in a gentle curve (the particular curvature of which depends on the construction of the generator), until at 9 reaches its highest point; thence it flows downward in a similar but reverse curve, until it again

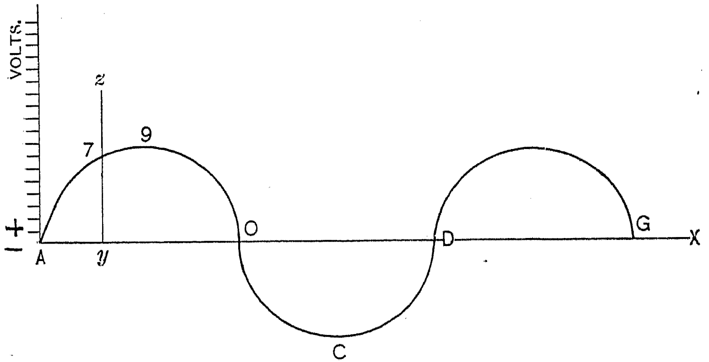

Fig. 1.

reaches the line *A X*, and, continuing downward, finally attains a sub-maximum at *C*. From this point it rises again, crossing the line *A X* once more at *D*, whence a repetition of the former curves occur.

The E. M. F, of the alternating current at any particular time can be determined by the distance of the curved line above or below *A X* at that time. For instance, suppose we wish to determine the E. M. F. at any time, *y*. Through *y* draw *y z* perpendicular to *A X*, cutting *A* 9 at 7. Then 7 *y* represents the E. M. F. applied to the circuit at the time *y*.

From an examination of the curve it will be seen that at stated intervals of time *A*, *O*, *D*, *G*, there is no E. M. F. applied to the circuit; and, if the curves above the line *A X* represent E. M. F.'s applied in one direction, those below the line *A X* represent E. M. F.'s in the opposite direction. Hence adjacent E. M. F.'s alternate in direction, and hence the term alternating currents.

There is a general misunderstanding to the effect that the alternate current dynamo is less economical than is the direct current generator. This is not so; there is a little difference, it is true, but the difference is, I believe, in favor of the alternate current machine. Let us take a dynamo field, for example, that of a simple direct current type of dynamo, and separately excite it with a constant current; let us now wind two armatures of identical dimensions, one for direct and one for alternate currents, and arrange to run them at the same speed; and let the windings of the two armatures be identical in size and number of turns of wire. Now the E. M. F. produced in these two armatures will be found to be, for the direct current armature;

E. M. F.	X volts.
Resistance of the armature	R ohms.
Amperes per section of winding	A amperes.

And because the armatures are identical we have for the alternate current dynamo:

E. M. F.	$2\ X$ volts.
Resistance of the armature	$4\ R$ ohms.
Amperes per section of conductor	$\frac{1}{2}\ A$ amperes.

Thus the direct current dynamo will generate $A\ X$ watts, and the alternate current dynamo $2\ X \times \frac{A}{2}$ or $A\ X$ watts also. Hence the capacities of the armatures are equal.

The loss of energy in the armatures will be, for the direct current dynamo, $A^2\ R$ watts. For the alternate current dynamo $\left\{\frac{A}{2}\right\}^2 \times 4\ R$, equals $\frac{4\ A^2\ R}{4}$, or $A^2\ R$ watts. From this we learn that the losses in the armatures are equal. It is stated, however, that owing to self-induction, Foucault currents, or something else, these figures do not represent the actual waste of energy. The waste of energy by self-induction in the direct current dynamo occurs when a brush on the commutator touches two adjacent strips at the same time, as in Fig. 2. The arrows indicate the direction of the induced currents and show that at that time all the E. M. F. of the short circuited coil is wasting

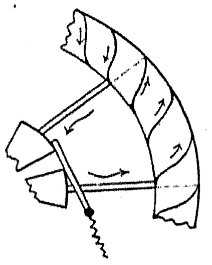
Fig. 2.

current on a short circuit through the brush. What now is the loss in the alternate current dynamo through self-induction?

In the direct current dynamo the current flowing through the armature coils magnetizes the armature with a polar line at an angle to the polar line of the field. These armature poles maintain a constant position relative to a fixed point, and are of a constant value. In the alternate current dynamo, the current in the armature rises above and falls below a zero value periodically; hence the iron in the armature has periodic polar tendencies at an angle to the polar line of the field.

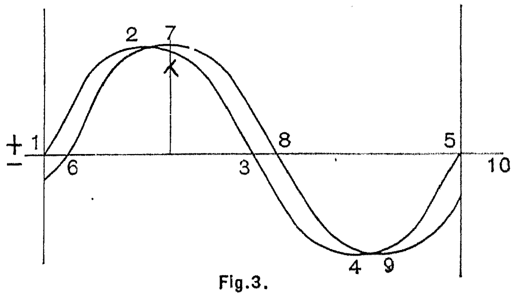

Fig.3.

I shall endeavor to show that instead of wasting energy by periodically polarizing the armature, the E. M. F. devoted to this purpose is at first temporarily stored and subsequently returned. As is well known, a certain period of time is required to establish a given magnetic charge in iron, which time is directly proportional to the degree of magnetization to be attained.

If, therefore, we plot a curve, the abscissæ representing time and the ordinates the degree of magnetization, and if we apply this curve to the curve of E. M. F. producing it, we have two similar curves separated from one another by a given period of time, as shown in Fig. 3, where 1, 2, 3, 4, 5 represents the curve of E. M. F., and 6, 7, 8, 9, 10 the corresponding curve of magnetization.

When the coil on the alternate current armature leaves a pole of the field (during rotation) the lines of force threading it rapidly diminish, and by induction generate an E. M. F. in the coil. The rise of E. M. F. and consequent flow of current in the coil is attended by a magnetic polar effect in the iron core, which not only increases while the E. M. F. is rising from 1 to 2, but continues to increase, attaining its maximum value when the E. M.

F. has fallen to *K*. Up to this time, namely, the time when the magnetic charge is maximum, energy has been expended upon the iron core; but notice, that at this time the charge on the iron core is superior to the charge producable by the E. M. F. at that instant; consequently, the charged core does positive work upon the E. M. F., if I may use the expression, and in doing this decreases its own value.

This auxiliary E. M. F. developed by the magnetized core upon the armature conductor assists the E. M. F. proper; that is, the E. M. F. developed by the field magnets. Thus the energy devoted to magnetizing the armature, during the rise of wave of E. M. F., is returned as E. M. F. during the fall of potential. We may assume that the wave is shifted along or retarded. Fig. 3 illustrates this retardation. 1, 2, 3, 4, 5, is the curve representing the position of a wave of E. M. F., without taking into consideration the effect of the iron core, while 6, 7, 8, 9, 10 is the curve-wave of E. M. F. which has been retarded, by reason of the magnetic charge on the iron from the theoretical position, 1, 2, 3, 4, 5.

The curve of E. M. F. of a coreless armature is quite different; here, as before, the theoretical curve is *A*, *B*, *C*, *D*, *E*, Fig. 4. Let us see what the effect of self-induction is in this case.

We know that the self-induction is proportional, among other things, to the current flowing at a given time, Now, at the time *A* no current flows; hence, there is no self-induction. But as the E, M. F. rises, and current is driven through the coil, the self-induction or counter E. M. F. prevents the E. M. F. from attaining its theoretical value. It does not delay the E. M. F. from its theoretical time position, as is the case with an iron core, but prevents the E. M. F. from attaining its theoretical

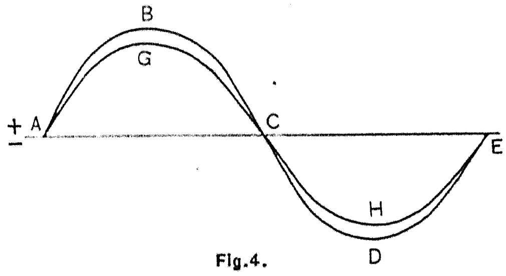

Fig. 4.

maximum value at any time. The real curve of E. M. F. in this case would be *A*, *G*, *C*, *H*, *E*, Fig. 4. This curve shows that there is no waste of energy due to counter E. M. F., but that the armature output is reduced by self-induction. An iron core, therefore, causes the counter E. M. F. to oppose the E. M. F. proper only during one-half a period, and allows it also to assist the E. M. F. proper during the other half period in the phase.

It is evident that the time of retardation, viz., the distance between 1, 2, 3, 4, 5 and 6, 7, 8, 9, 10, Fig. 3, depends on the quantity of current flowing through the armature coils as well as on the core. I have described this point at length, as there is a general, although false, impression that the iron core of an alternating armature necessarily wastes energy.

We may roughly compare the theoretical efficiencies of the direct and alternating dynamos, as follows The output is equal; the waste of energy in armatures due to $C^2 R$ is equal. The waste due to self-induction is greater for direct than alternating dynamos, with properly constructed iron cores.

Faraday's law of induction may be stated as follows: The variation of the magnetic field surrounding a conductor induces an E. M. F. in that conductor. A variable field is produced by a variable current, and hence, a conductor carrying an alternating current will induce an E. M. F. in another conductor residing in the same field.

Let us note the values of the E. M. F.'s in the inducing or primary, and induced or secondary circuits, at the same time, and first when no current is flowing. The curve, *A*, *B*, *C*, *D*, *E*, Fig. 5, represents, as before, the alternating applied E. M. F. Now, the induced E. M. F. will be a maximum at the time when the inductive effect of the primary current is a maximum, and the inductive effect of the primary will be greatest when its E. M. F. is changing most rapidly. Evidently this will occur near the zero line *A X*; for the E. M. F. will vary the most when passing from a finite value to zero, or from zero to any finite value. Hence the inductive effect of the primary and the E. M. F. developed on the secondary circuit will be a maximum when the primary E. M. F. is least.

Fig. 5 represents the time relation of the primary and secondary E. M. F.'s when no current is passing. The position of maximum E. M. F. of the secondary is, as you see, one-fourth of a complete period behind that of the primary. Now, a moment's consideration will show that the secondary circuit is simply the receptacle of an E. M. F., and, when no current is flowing in it, plays no part in the inductive phenomena.

The primary circuit evidently will induce in itself an E. M. F. in the same manner that it impresses the E. M. F. on the secondary circuit. Hence, in any circuit to which is applied an alternating E. M. F., we have two phases of E. M. F.; the one, the applied; the other, the self-imposed; the points of equal value in each being distant from each other one-fourth of a complete period, when practically no current is flowing. This self impressed E. M. F. is called "self-induction," or counter E. M. F. When two or more coils are wound together so as to act collectively, the effect of the one upon the other is termed "mutual" induction. We now see why, when an alternating E. M. F., of, say, 1,000 volts, is suddenly applied to a coil whose resistance is low, as, say, 1 ohm. the coil does not receive the quantity of current destined for it by Ohm's law. The kinetic E. M. F. is very small because it is the difference between the applied and counter E. M. F.'s.

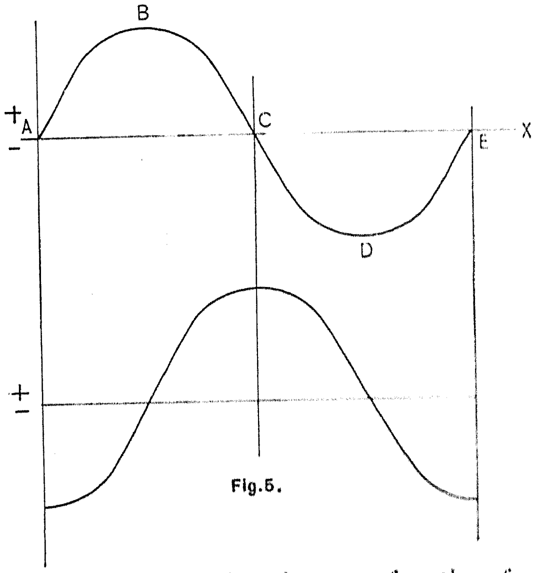

Fig.5.

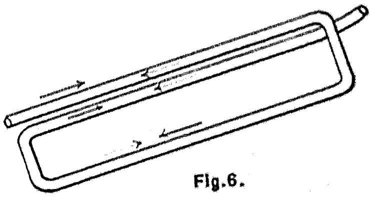

Fig.6.

Fig. 6 shows a wire wound on itself or coiled. The full arrows indicate the direction of the applied E. M. F., the dotted arrows the counter E. M. F. In the two adjacent turns one impresses a counter E. M. F. on itself and also a counter E. M. F. on the other and the other impresses a counter potential on itself and on the first. Hence in two coils of wire we have four impressed counter E. M. F.'s. This is commonly stated as the self-induction; and is proportional to the square of the number of turns.

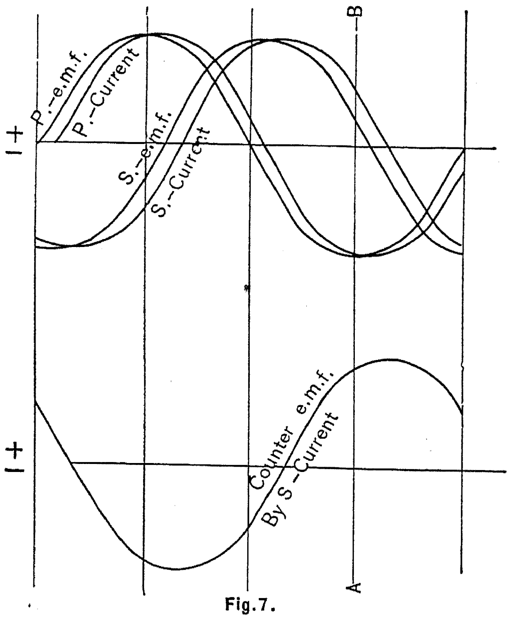

Fig. 7.

We will now consider the time position of the currents flowing through coils impelled by the E. M. F.'s of which we have spoken, and endeavor to apply the effects of the various positions they assume to a few of the phenomena noticed in working induction coils with different current strengths.

Evidently, when little current is flowing over a conductor, the self-induction and retardation are small; hence the curve of the current nearly coincides with the curve of E. M. F. Fig. 7 illustrates the positions. Here the phase of the primary current nearly coincides with the phase of its E. M. F. The curve of the secondary E. M. F. is, however, just one-fourth of a period later than the primary current phase, while the curve of the secondary current is still farther behind it; so that, while the primary current phase is x distance behind the primary E. M. F. phase, the secondary current phase is $2x$ plus one-fourth of a period behind the primary E. M. F. phase. This delay reacts on the primary E. M. F. The lower curve in Fig. 7 is the curve of the counter E. M. F. developed by the secondary current upon the primary circuit. It will be noticed that it no longer exactly opposes the phase of the applied E. M. F., for the time line *A B*, passing through the maximum position of the primary E. M. F., cuts the counter E. M. F, curve at a point before its maximum value is attained.

Fig. 8 shows an extreme case. Here the curve of the primary current is one-eighth of a period behind its E. M. F. Consequently, the curve of secondary current is one-half a period behind the primary E. M. F., *i. e.*, $\frac{1}{8} \times 2 + \frac{1}{4}$; and the curve of counter E. M. F. developed by the secondary current is $\frac{3}{4}$ of a period behind the primary E. M. F.

We are now prepared to understand why the counter E. M. F.

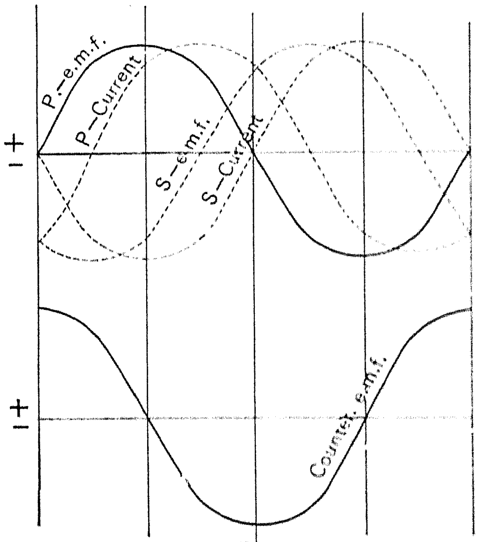

Fig. 8.

developed in the induction coil varies in its counter effect as current is abstracted from the coil. The reason is simply that its position of counter effect is dependent on the curve of its producing current, and as the current occupies a variable position relative to the applied E. M. F., the counter E. M. F. successively occupies positions of lesser counter effect until eventually it has no counter effect whatever.

These phenomena are taken advantage of in the operation of induction coils. If a constant E. M. F. be applied to the primary circuit of a converter, successive increments of current will be allowed to occupy this primary circuit as successive quantities of current are abstracted from the secondary circuit; because these currents push along out of the way the opposing counter E. M. F., and consequently the kinetic E. M. F. attains correspondingly greater values.

Thus far we have considered only two factors of self-induction and retardation, viz., the number of turns of the conductor, and the quantity of current flowing; but there is another factor, most important, and that is, the resistance of the magnetic circuit.

The magnetic resistance of iron is, as you know, similar to the resistance of any other conductor, in that it is directly proportional to the length, and inversely proportional to the sectional area of the circuit. This, however, is only true for limited values of magnetism. As the magnetic lines traverse a core they encounter a resistance proportional to their number at first; but if more lines be continuously added, a number is eventually reached which the iron utterly refuses to carry. This degree of magnetism is termed "saturation."

It can be shown that the position of the curve of counter E. M. F. is directly connected with the degree of saturation of the core, so that with a given coil the position of no counter effect of the counter E. M. F. coincides with saturation of the core. The magnetic resistance of the core of an induction coil determines, then, the limit of retardation or lag of the current phase. Hence, if we wish to construct a coil in which the counter E. M. F. shall always be inversely proportional, and the kinetic, applied E. M. F., always directly proportional to the current abstracted from it, we make the core of the coil with the lowest possible magnetic resistance. Such a coil, when supplied with a constant difference of potential, will furnish at its secondary terminals an approximately constant E. M. F. It is also evident that a coil having a limited section of core supplied with a constant current may deliver a constant E. M. F. on its secondary terminals, provided the field produced by the constant current is sufficient to saturate the core.

I noticed these phenomena several years ago when experimenting with a series system for lighting. Mr. O. B. Shallenberger has applied this saturation effect in a most striking manner.

He connects a number of incandescent lamps, say four, in series, attaching their outer terminals to a source of constant alternating E. M. F. Fig. 9. Around each lamp is shunted a self-induction coil, so proportioned that an E. M. F. of say 100 volts will saturate the core of the coil. If, now, a lamp be turned off, its current is diverted through the shunted coil, which offers a counter E. M. F. of 100 volts. We would naturally expect that the light in the remaining lamps would grow dim, but this is not the case. If we measure the difference or potential on the extreme terminals of the lamps we find it to be 200 volts; on the three burning lamps it is 50 volts each, while on the shunted coil it is 100 volts; so that the sum of the potentials of the three lamps and the coil is 250 volts, while the applied E. M, F. is but 200 volts.

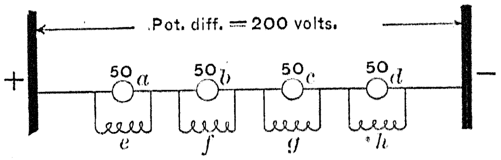

Fig 9.

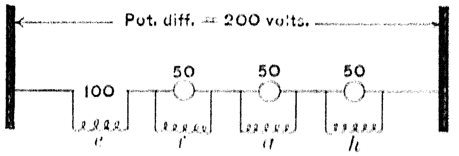

Fig. 10.

I think the explanation of this is as follows: During the first part of the rise of the E. M. F. wave, the shunt coil practically offers an infinite counter E. M. F., in comparison with which the resistance of the lamp may be neglected; when, however, the applied E. M. F. has attained a value of 100 volts, this counter E. M. F. is surmounted, because the core of the coil is saturated by the current flowing in the coil.

The lamps, therefore, receive the remaining 100 of the 200 volts applied, since the coil, being saturated, offers no additional counter effect. Now, after the wave of E. M. F. has declined to a value less than 100 volts, the stored magnetism in the core of the shunted coil begins to lose strength, and in so doing, develops an additional E. M. F., which, assisting the generator current, furnishes a current during the time the applied E. M. F. is of low, or zero, value, and by so doing keeps the average E. M. F. on the lamps constant.

It is necessary then, in working with alternate currents, to take into consideration the time retardation or lag of the current behind the E. M. F. We have seen that this lag is dependent on the resistance of the core, and on its degree of magnetization. I have found it especially necessary to make time corrections in work on alternate current motors, and in apparatus depending on the attraction or repulsion of magnets energized by alternate currents.

I have stated that there is no loss due to the periodic magnetization of iron cores by an alternating current. This is only true of a perfectly subdivided core. A solid core allows secondary currents to flow. Hence its storage capacity is lowered. In practice, the loss due to wasted currents in laminated cores can be kept as low as one per cent.

We may summarize the effects of the retardation of currents behind the E. M. F.'s in the induction coil as follows:

Retardation causes the counter E. M. F. developed to have counter values inversely as the quantity of current flowing in the coil.

Retardation causes the energy expended in magnetization during the increase or development of a current to be returned as current energy during the subsidence of the current.

To an alternating or similar current, iron offers true storage capacity for all values of magnetization below the saturation limit.

DISCUSSION.

THE PRESIDENT:—Discussion is now in order. Should there be any gentleman present who is not a member of the Institute, and who is wishful of taking part in the discussion we should be very glad to hear him.

MR. TOWNSEND WOLCOTT:—I would like to ask Mr. Stanley if he has ever observed an effect in reversing the magnetism of the cores other than that of the Foucault currents, that is, a sort of magnetic friction; the mere reversal of polarity, independent of any induced current, producing an amount of energy. I do not know how much it is. I know that some years ago a French physicist experimented with alternating currents, using a copper core in one case and an iron core in another. Had the heating effect been due to the Foucault currents, the copper would have become much hotter than the iron, while in fact it did not get hot at all while the iron became hot; so there was an effect due to the simple change of polarity—a sort of friction:—it is analogous to friction. I would like to know how much effect that has in practice on the alternating systems.

MR. STANLEY:—In measuring the loss in an induction coil we have found a very slight unaccountable waste, so slight though, as to be almost immeasurable. It depends somewhat on the number of alternations that are driven through the coil. With about sixteen thousand alternations a minute the temperature of the coil, if it were placed in a calorimeter, would be indicated. The temperature of the calorimeter would indicate very nearly the amount of energy which was due to C^2 R, or the heating of the conductor itself. The iron of the core of the converter or the induction coil remains at a very low temperature indeed. Of course there is a considerable radiation while in the air. Mr. Shallenberger in Pittsburgh has made several careful measurements and we are enabled to find that there is hardly any perceptible loss from magnetic friction. I can well see that with a very high degree of magnetization where the core was of a limited area in proportion to the number of ampère-turns surrounding it, that there might be a considerable waste due to magnetic friction or magnetic saturation, and with the induction coils as made for the distribution of electricity for lighting purposes this is very small indeed. I cannot tell you its extent, but it is very small.

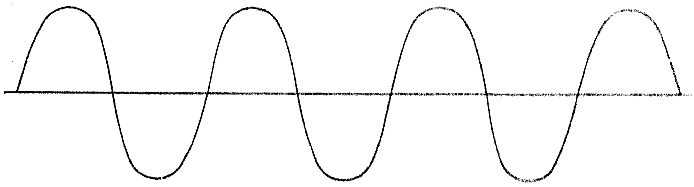

Fig. 11.

MR. WOLCOTT:—The one per cent. you speak of would include that, as well as the Foucault current?

MR. STANLEY:—Yes, sir.

MR. HUBERT HOWSON:—In connection with this subject it occurred to me it would be of very great interest if we could have a precise definition of the term "alternating current." In looking into this subject it seems to me the phrase has been used to describe different characters of currents, and possibly in the course of a very short time it may be found necessary to have a correct definition of this term. Perhaps I can explain my meaning by a diagram. As commonly understood a graphic representation of an alternating current, is I take it substantially what Mr. Stanley has given us in figure 7; but I have also found the term used to express a current which I should suppose was rather to be more correctly termed a "reversed current." To give a graphic representation, we will say that figure 11 is what we ordinarily understand as an alternating current. Some of you know very probably that, in connection with the subject of distribution by means of transformers, it has been proposed to substitute for the alternating current generator, a generator which shall give a continuous current and which shall have in connection with it a commutator which shall reverse the current, and I take it that a graphic representation of a current produced in that manner would be—giving these time values—substantially as shown in figure 12. I have also seen the term "alternating current" used by

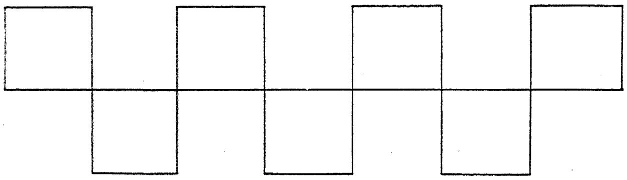

Fig. 12.

Mr. Stanley himself, I think, in connection with the description of one of his inventions—a very ingenious one:—It was a system of distribution in which he proposed to use also a continuous current generator, and he proposed to use a transformer which should automatically convert that continuous current into what he termed an alternating current. The device he used was a transformer in which he had a core, two primary wires and one secondary wire. Of the two primary wires, one had a continuous current. The other primary wire was in a shunt and had an automatic attachment, by which its circuit was broken and closed again. The result of that was, he said, that the core was magnetized and demagnetized very rapidly, and he called the current that was produced in the secondary an "alternating current." It seemed to me that a graphic representation of that would probably be as shown in figure 13. Now are we to use the term "alternating current" as applied to one or to all of these three things? That is the question which passes through my mind.

Mr. Stanley:—If I called the current produced, as Mr. Howson has suggested there in the last figure, an alternating current, I was certainly wrong and it is of very little value for the purposes of induction. The maximum electro-motive force is induced at a time when the current is changing most rapidly. Now the current does not change most rapidly between the points A and B, (fig. 5) but most rapidly from any point of any value, of any electromotive force whatever, to zero, and consequently we cannot get the same number of volts induced per foot of wire on the converter or induction coil with this form of current that we get with a truly alternating current. It is necessary that the current should absolutely pass above the line and go below zero and rise above the zero line again, having plus and minus alternating values in order to get a maximum inductive effect. There will be residual magnetism no matter how successfully the apparatus is made to work. Necessarily any current of one sign—any plus current or any minus current,—must develop in any given time some residual magnetism in the iron, and consequently the secondary current cannot have as great value as it would have without that same amount of electromotive force, half above and half below the zero line.

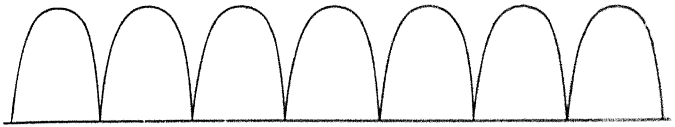

Fig. 13.

Mr. Schuyler S. Wheeler:—One point I think was passed over in reviewing that sketch on the board. The current drawn there as an induced current was an impossible one as far as an induction coil is concerned. If the first upward stroke or line in igure 13 represented the making of magnetism it would generate say a positive current like first upward stroke in figure 14. Now, when that magnetism was broken or ceased, as represented by the downward stroke figure 13, it would generate a negative current, which would be represented by a curve *below* the horiontal line in figure 14, whereas the figure as drawn represents

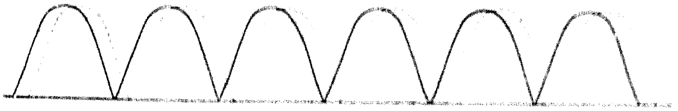

Figure 14.

only a succession of positive impulses. You cannot get impulses of positive current only, in an induction coil, and the drawing is simply an impossible hypothesis.

Mr. Howson:—My point was that the polarity of the core in that case was never reversed. It was simply magnetized and demagnetized by the make and break of the primary current which I had in mind in sketching that diagram.

Mr. Wheeler.—The demagnetizing would produce a negative current.

Mr. Howson.—I would be very glad if Mr. Stanley would draw out the curve of the current produced by the apparatus; if he has experimented sufficiently with it to enable him to do so

Mr. Stanley.—Well, to begin with, we have a certain definite electromotive force applied to the apparatus as represented (Fig. 15). As that electromotive force is constant there is no induced current at that particular instant of time. Now if that electromotive force be varied, no matter whether it be open circuited or short circuited, or however it be—at this particular instant of time, there will be—at the instant of withdrawal, an induced electromotive force of a positive sign rising in opposition to the electromotive force proper, as shown by the line D. E. If we have an electromotive force of A. D. volts applied to a coil, on the withdrawal of this electromotive force, either suddenly or gradually, as shown by the curve A. B. Induced electromotive force will rise at the same time. This would be the applied electromotive force the other the induced electromotive force at that time. Now if the circuit be closed again, and the current developed as in the case B. C., the induced electromotive force at this time will be, I think, as

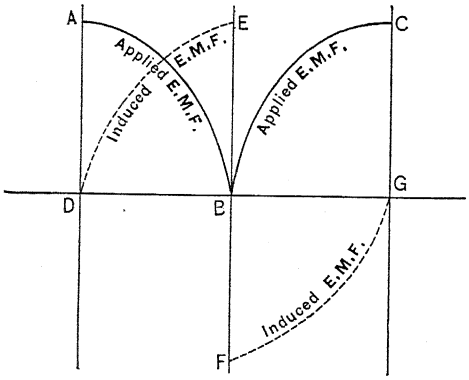

Fig. 15

shown in the curve F. G.. During the development of the electromotive force or current the induced electromotive force will be of opposite signs. During the fall of the electromotive force it will be of the same sign. And I think you will find, if you plot it out, that the difference of equal value between applied and induced electromotive forces will be just one fourth of a complete period—that being called the period of complete alternation, if it were an alternation apart.

DR. P. H. VANDER WEYDE:—To properly understand those currents we must draw two entirely different lines; one curve representing the rise and fall of the electromotive force; another curve representing the positive and negative of the induced current. When the primary current is only rising and falling, it is all of one sign—it is either all positive or all negative. It comes to the neutral line and then rises. Now, we can, if we only interrupt the current, when we have a current of the same sign—the primary (the secondary is always of different signs; the secondary is always positive and negative whether the primary is or is not)—but if we cause the primary to alternate too, we increase the electromotive force of the induced current. We get, by the fall of the current in the primary wire—we get the current in the same direction in the secondary wire; but if in place of merely interrupting the current, we change the sign, that secondary current is increased—by the primary current in opposite direction. I made a series of experiments several years ago, and I was induced to prosecute them. They were very encouraging and very interesting. I had constructed a relay with two coils—a primary coil to receive and transmit the current, through a secondary coil over another circuit. The first experiments gave me very curious results and I have extensively continued it and finally come to the construction of several forms of transformers and very peculiar forms too. I found at last that it was of the utmost importance, if we wished all the benefit of our primary current, to close the magnetic circuit, as I call it—to put pole-pieces to the cores or connect the cores. If we put a pole-piece to the cores we get, at the first break of the contact of the primary current, a very strong secondary current; but it is only at the first break of the contact. The second break is weaker—much weaker, and I found that it lost considerable magnetism. When we open the armature again, then we get the secondary current.

What was to be the remedy of that? I found soon that the only remedy was to be the reversion. We cannot repeatedly break the armature contact, but when we reverse we have the full effect—not only that—but we have the double effect of the reversal of the polarity of the core. When I found that fact I made a series of experiments, always with measurements with the volt-meter to see how many degrees of difference there were. I had constructed a volt-meter, which I arranged so that ten degrees represented a volt. At the same time there was an ammeter connected with it which was constructed similarly. The first experiments were to remove the core and substitute a solid piece of iron which had more than double the weight of the original core. I elongated the solid core. I put in a 24 inch core. Then I commenced again with wires and I took a piece of wire for a core 6 inches long and I took a piece of wire 24 inches long and I found that one piece of wire gave quite a deviation and half a dozen pieces of wire gave more deviation than the whole bunch of 280 wires which had formerly filled it. Among other interesting experiments was the following, shown in figure 15 A I used 280 soft iron wires for the core and then I bent them around. I took half of these wires and bent them around one way; then I took the other half and bent them over on the opposite side and in that way surrounded the whole and interlaced the wires well. Then I had a coil on which at the first contact we had a very strong induced current about four times the intensity which you could obtain otherwise. But then I found the necessity of reversing. When I reversed the current then I got about four times the intensity of the current. I have such a coil with me, which I would be glad to exhibit to the gentlemen present.

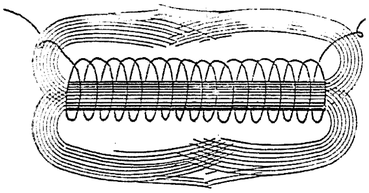

Fig. 15 A.

Mr. Geo. B. Prescott, Jr:—I would suggest that Mr. Stanley get the date of the invention from Dr. Vander Weyde, as that is the induction coil now manufactured by the United States Electric Lighting Company.

Mr. Wolcott.—As the electromotive force falls off as Mr. Stanley stated, it would generate electromotive force of the same kind in the same direction in the secondary. I distinguish here between electromotive force and current. Of course the current would not follow on account of self-induction—would not follow electromotive force, just as he has shown in some of these diagrams here. If that fell off it would make a negative electromotive force in the secondary, and as it rose again the direction of the primary current is reversed and also the direction of the secondary electromotive force with respect to the primary. But of course if the wires were a complete circuit so that we could have a current, the current would vary very much from that. As I take it, that is very similar to the case of a Rhumkorff coil in which the current is exactly alternated. The conditions can be so fixed that the current in closing the coil is very small—the electromotive force is low at least. In no case is it anything like the electromotive force of breaking the circuit of a Rhumkorff coil.

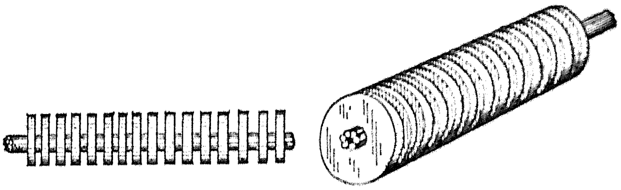

Fig. 16.

Mr. Stanley.—With your permission, I will endeavor to sift out this subject a little bit more. As I understand it, the effect of joining the magnetic circuit around the primary and secondary induction coil is practically to reduce the wire, if I remember correctly, to about one-sixth. In experimenting with coils, to begin with, I took Faraday's ring and wound a coil on either side. The best coil I have ever seen consisted of a bundle of straight wires on which was threaded a lot of iron washers (figure 16.) This has the highest counter induction per unit of length of any coil I have ever seen. In the Westinghouse Company's system we get about two volts per foot of wire. This coil gave, if I remember correctly, about a volt to four inches. But it was impracticable to construct, as they could not make them fast enough. As I understand it the curves of electromotive force induced from an alternating current are these, Fig. 5. *A*, *B*, *C*, *D*, *E*, is a complete period of the curve of the primary or applied electromotive force. Now the induced electromotive force will be maximum at the time when the inductive effect of this primary current is a maximum; and the inductive effect of the primary current is a maximum when it changes from zero to any larger value. Consequently, the electromotive force of the induced current shown in the lower curve, figure 5, will be a maximum at this time; and it will be a minimum when the inductive effect of the primary is a minimum. and that will be when the rate of change of the primary is the least, viz., at the crest of the primary wave *B* and *D*, we find that the same values of applied and counter electromotive force are just one-fourth of the whole period of phase of the current. That is with an alternating current. In the direct current we never can have as great an electromotive force induced in the secondary circuit because our primary circuit never passes the zero line. The curve of induced electromotive force in the case of a broken, direct or interrupted direct current I think is this, Fig. 17. As will be seen, the induced current will be of negative sign, while the positive current is going to

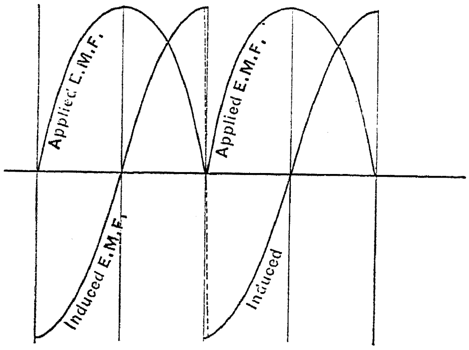

Fig. 17.

maximum; and the induced current will be of positive sign, while the applied current is falling from the maximum.

Mr. Wolcott.—How do you get from the positive to the negative there?

Mr. Stanley.—The rate of change at the break is enormously rapid.

Mr. Wolcott.—It is infinite according to that.

Mr. Stanley.—It is almost infinite. If the severance of a contact requires no time whatever then it would be a straight line; if not it would be a little inclined.

Perhaps it might be interesting to illustrate the applications of induction coils for covering long-distance distribution. It is well known that the cost of a conductor varies inversely as the square of the electromotive force. The cost of the conductor for transmitting a given amount of energy will be, for 100 volts E. M. F., we will say, x dollars, while for a 200 volts pressure the cost will be $\frac{x}{4}$ dollars. Therefore, the economy of the alternating system over others is directly as the square of the ratios of their electromotive forces. But in order to cover enormous distances and to make the system as broad as possible, I devised another plan which may or may not be new. We are to apply it in Berkshire County, Mass., to distribute from a station over 6 miles of territory with a loss of 2 per cent. We have 100 horsepower to distribute and the potential of distribution is to be 4,000 volts. Our alternating current dynamo is of 1,000 volts. In

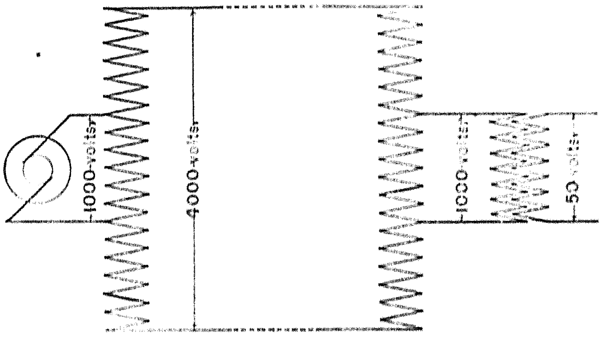

Fig. 18.

order to get the electromotive force up to 4,000 volts we simply wind one coil of wire and attach our dynamo to intermediate terminals of that wire, distant apart one-fourth of the entire length, as shown in figure 18. That is, the length included between the terminals is one-fourth of the whole length of wires wound on the coil. There is no secondary circuit whatever. Of course there is an induced electromotive force upon every turn of wire on that core of so many volts per turn. We apply our electromotive force of 1,000 volts at those points in order to get 4,000 volts between the outside terminals. When we carry our current at this enormous potential over to the place where we wish to distribute we simply attach another self-induction coil similar to the first. On one-fourth of its entire length we tap out another circuit. That reduces the potential from four thousand volts to one thousand volts again. The maximum efficiency of an auto-converter, or converter of one coil, evidently is found in the case where the ratio of reduction is from 1 to 2, or 2 to 1. If you have it in the ratio of 1 to 4, or 1 to 6, the saving is not nearly as great.

MR. PRESCOTT.—I visited the station in the Berkshire Hills, that Mr. Stanley speaks about, a few days ago, and I noticed that they had for the generators one omnibus wire running around the station such as are used in the Edison stations. But this omnibus wire was split in two, and each generator worked into its own circuit. I asked the attendant why the omnibus wire was not connected across and he said that if they connected it across they were likely to burn out their armatures. I would like to ask Mr. Stanley if that is so?

MR. STANLEY:—That is so, through an oversight in the construction of the dynamos in the early stage of the business. We all have to learn some things; and the reason why it has been difficult to multiple-arc alternating dynamos is, that the phases have not been exactly the same. We made our dynamo pole pieces of cast-iron. Those cast-iron pole pieces did not measure to a thousandth of an inch the same width, and consequently the two phases of current, which ought to have coincided, did not coincide exactly, simply because of faulty construction. By tooling off and cutting the width of the pole pieces to an exact measurement, there is not the slightest difficulty in multiple-arc-ing two, three or four alternating dynamos. I think a new feature in alternating currents, perhaps not well known to every one here, is the remarkable self-regulation of the alternating current machine. I cannot speak of it fully, because I have had very little to do with it. Another man is responsible for the beautiful work he has succeeded in getting. In Pittsburgh, a few days ago, I saw a 2,500-light dynamo run by a 250 horse-power engine, and 2,150 lights thrown by a single switch. The rise in electromotive force was one volt. I submit, that in the present state of the art, it is impossible to construct a direct current machine which shall have anything like the self-regulation these alternating machines exhibit. The resistance at the commutator of a direct current machine would cause a greater variation of potential than was manifested in this machine I speak of. In the induction system we start out with an alternating current machine, in which we pretend to have at least a maximum loss of two volts in electromotive force in the dynamo with the proper regulation necessary to any machine. Towns are wired, at least I know of two pole lines which are wired, for one per cent loss of electromotive force, distributing electricity generally over an area of about a mile radius. The actual loss in the mains is one per cent. In the induction coil, at full load, there is another loss of two per cent.

Mr. Mailloux :—I was expecting that somebody would refer to one of the features mentioned by Mr. Stanley this evening, which is to me one of the most interesting, from the theoretical as well as from the practical standpoint, inasmuch as it leads to a score of applications. It is that system in which four lamps, *a*, *b*, *c*, *d*, were connected across the terminals of a constant potential alternating circuit; and in which this difference of potential for a particular lamp cut out was taken up by the self-induction coil as *c*, figure 10. Now I would like to ask Mr. Stanley whether the object of using that induction coil was merely to balance the other lamps, or whether it was expected that any energy would be saved by its use; in other words, whether the two lamps running with the circuit continued through the self-induction coil would require less energy than would the four lamps.

Mr. Stanley :—The connection of the lamps in series in that way was for a street-lighting system. In order to distribute lamps economically for street-lighting purposes the plan adopted by Mr. Shallenberger was brought out. The shunted coil on the extinguishment of any one lamp prevents the rest of the lamps going out. For instance, there would be twenty lamps attached to a 1,000-volt circuit, each lamp of 50 volts. Now all those twenty lamps may go out, or nineteen go out and the remaining lamp would be at approximate candle power; and yet the sum of the separate electromotive forces, measured by a volt-meter around the lamps and coils, is far in excess of the electromotive force that is applied to the outside terminals of the series.

For instance, there are four lamps connected in series. There was a lamp connected at *a* Fig. 9. That lamp is extinguished and in place of it there is a self induction coil *e*, Fig. 10. This series is connected to a circuit whose E. M. F. is two hundred volts; a volt-meter connected to each one of these lamps will show for the lamps *b*, *c*, *d*, 50 volts each, and for the self induction coil, 100 volts. Now the sum of the interior electromotive forces here is two hundred and fifty volts. The same volt-meter will show an electromotive force of two hundred volts on the outside terminals. That is, the sum of the interior electromotive forces is greater than the applied electromotive force. I think that can only be explained by the retardation of the current producing the average electromotive force.

MR. PRESCOTT:—In regard to the point that Mr. Stanley kindly replied to a moment ago, I would say that it is of course well understood that if the time phases of any number of alternating current machines are identical there is nothing to prevent their being worked in multiple-arc. Now the question to my mind was whether there is any practical difficulty in getting those time phases the same. Of course that includes not simply the electrical and mechanical construction of the machine; it requires them to be identical in that respect. Now I have seen only two Westinghouse stations, and in both of them, each dynamo had a separate engine and in both cases the machines supplied separate circuits. Is there any case of any plant now in operation where a number of machines are successfully used in multiple arc?

MR. STANLEY:—I think at Denver, Col., a station of five thousand lights is run with dynamos in multiple-arc, although I am not sure. The Pittsburgh station has been run in multiple-arc. Our policy is not to run in multiple arc. We prefer not to. There is no choice in the matter excepting only that of simple expediency. We construct the station switch-board so as to connect any circuit leading from the station to any dynamo without causing a flicker in the lights, and thus give the dynamo a definite load that you can read on the ampèremeters. That seems to us to be a very satisfactory way to run. As the evening grows later lights are cut off, the circuits are transferred from a number of dynamos to perhaps a smaller number, and eventually down to one dynamo which runs all night.

The subject of multiple-arcing of the dynamos comes so near the question of running alternate current motors that it is the same question practically, and I feel, perhaps, justified in explaining a little phenomenon in that. If you take two alternating current dynamos and couple them together, and start them up, one dynamo will fall into sychronism with the other. In order to have an alternating current motor, it is necessary to have a direct current through the field and an alternating current through the armature. In order to get a direct current through the field, you have got to have a commutator, which will redirect the field current. This current in the field will rise from zero to a maximum value and fall again to zero value, but the armature currents will be above and below the zero value, as shown in Fig. 19. Now, notice, that at the time marked by the full vertical lines, there is no current flowing through the field, and there is no current flowing through the armature. If, then, we have an alternating current motor which is running with the exact speed and in conformity with the number of alternations of the dynamo which is applied to it, and have the phases of the two currents in exact synchronism, that is, with equal phases coming together at the same times, we will have absolutely no current in the field while there is no current in the armature of the motor. Now the commutation of the field may be outlined as shown where *C C C* indicate the commutator segments. There has been, until the motor arrived at synchronism, commutation

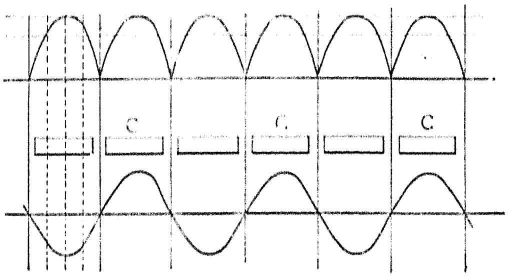

Fig. 19.

at different points in the phases as at the dotted vertical lines. But when the motor runs at exactly the same speed as the alternating dynamo, there is no force then at work keeping the motor going. In other words, you can run an alternating current motor up to speed, but the minute it goes into absolute synchronism it will fall off again, and the consequence is that the alternating current motor will see-saw. There will be no power to continue its motion. Its speed will fall, and it will drop down to a lower number of alternations, but will rise up again.

Mr. Prescott:—What is the effect if you have just enough work on to keep the speed down?

Mr. Stanley:—Then it will never rise to synchronism.

Mr. Rice:—I would like to ask Mr. Stanley in reference to his last statement as to the motor, what percentage is the variation in speed?

Mr. Stanley:—Of course an alternating current motor in synchronism and kept there, runs at exactly the same speed as the alternating current dynamo does. If the motor, is impelled to synchronism and beyond it, it will go to perhaps double the synchronism. If the alternating current machine is run 16,000 alternations per minute, the alternating motor if it goes beyond that number, would never stop until it gets to 32,000 alternations. But the chief difficulty is that it does not develop any counter electromotive force under the conditions described, because it cannot develop a counter potential unless it has at all times a constant field charge. As the current falls to zero, of course, it cannot develop any counter electromotive force at that time. An alternating current motor has no efficiency of any consequence until it arrives at synchronism and becomes practically a dynamo running in opposition to the dynamo applied to it, at which time it has an enormously high efficiency with the most perfect self-regulation.

Mr. Rice:—I hardly understand what Mr. Stanley means, in view of some experiments we tried recently. We had a self-exciting dynamo, and ran it in multiple arc with another self-exciting dynamo, and then threw the belt off. The current from the generator proper passed from the armature of the motor. We did not find any variation to speak of, and it ran perfectly in synchronism. It was a generator capable of giving out about twenty-five thousand watts. I then loaded it until the motor was giving out in the neighborhood of fifty thousand or sixty thousand watts without any change whatever in speed or any tendency to variation.

Mr. Stanley:—It had an alternating field in which the magnetization periodically fell to zero. But in the dynamo the gentleman speaks of, I understand, the magnetization of the field was always the same. Consequently it was a true alternating current motor.

Mr. Rice:—I do not see exactly how that meets the question, because the field of the motor was charged by itself, therefore, according to your statement, it was falling from zero to zero again.

Mr. Stanley:—The field of the motor was charged by a current developed by the motor itself on its own armature.

Mr. Rice:—Yes, sir.

Mr. Stanley:—And the phase of the current did not coincide vith the phase of the applied electromotive force.

Mr. Rice:—If I understand you for a certain period of time, vhen a motor running is exciting its own field, you say there is 10 opposition to the primary current from the generator?

Mr. Stanley:—That is true. When the current in the field s zero, and the field charge is zero, then there is no electromoive force producible of course. But if the field charge is contant, then it will develop a counter electromotive force and ecome a true motor.

Mr. Rice:—I understand you to say that the field would fall zero in a self-exciting motor.

The President:—I would like to ask Mr. Rice if he is at berty to give us a little information on the point of synchronism?

Mr. Rice:—All that I can say in regard to that is that we ave tried multiple-arcing dynamos. We have run dynamos gether for a period of twenty hours continuously and the loads emed to be very equally divided, when the speed was kept uniorm. We have also experimented with motors and found no ifficulty in that direction.

I was very much interested in the system which Mr. Stanley tributes to Mr. Shallenberger. I think it is only just to Prof. lihu Thomson to say that that system was patented by him ree or four years ago. He used almost identically that same rangement, which consisted of placing a self-inductive coil in lace of a lamp, so adjusted that it would take just the current of e lamp. I do not know that that was the first time it was ne, but so far as I know it was.

In reference to Fig. 4, I do not know that I correctly understand Mr. Stanley's explanation of it. The diagram, I understand Mr. Stanley to say, applied to a coil in which there were iron cores. Is that true?

Mr. Stanley:—Yes, sir.

Mr. Rice:—And you say that the self-induction follows exactly the primary. Now it seems to me that coils with iron or without iron are the same excepting a difference in degree; there is no difference in kind. In that case it seems to me there would be a slight shifting.

Mr. Stanley:—The difference in ratio is one to many thousands, and it being so small I thought it was not worth while expressing it.

Mr. Wheeler:—I would like to ask Mr. Stanley whether there are any losses on the line wire other than that due to resistance—whether there is any current dissipated by retardation, self-induction, or anything of that sort?

Mr. Stanley:—I was perhaps the worst frightened of any man who ever stood on solid ground when I read Prof. Hughes's lecture on self-induction. But the effect of self-induction which he described, and the measurements on which it was based, were made with a broken direct current. With the system that we use we have not been able to find any variation from the true Ohm's law, all our wiring details are based on the assumption that the resistance is the ohmic resistance of the copper only. In the case where the wire goes around an iron core, it is not necessary that there should be a loss of electromotive force. The electromotive force is merely retarded for a moment, and as soon as it has fallen to a value below that of the charged magnetism, the stored electromotive force is all returned to the circuit. I think there is nothing so generally misunderstood as the loss of electromotive force on iron cores.

Mr. Wheeler:—It is a case of magnetic storage.

Mr. Mailloux:—I do not think Mr. Stanley exactly answered my question, and the remarks made bring us back to it again. The question is whether that circuit that he speaks of in which an induction coil is substituted for a lamp, whether the loss of energy, in other words, the expenditure of energy, across that line is different in the second case from what it would be before that induction coil had been substituted. If, as Mr. Stanley argues, the effect of self-induction is simply to retard the electronotive force, then evidently we still have electromotive force here, and if it does not absorb electromotive force or cause oss, then we should take less energy than would be accounted for y that which would be absorbed in the three lamps, plus that bsorbed in the ohmic resistance, as he calls it, of the self-induc ion coil. For we must remember that the amount of energy equired at the terminals of the induction coil is not as much as ould be represented by $C^2 R$. It is $C^2 R$, plus a certain quantity which represents the work done in the induction coil in over oming the counter-electromotive force of the coil. Now, then, ; is interesting for us to know whether in that case the expendi ure of energy was the same whether the induction coil was on, or hether the four lamps were burning at one time. I incline to e opinion that it must have been the same, and that consequent there must have been a certain amount of work which was ab orbed and dissipated by the self-induction coil.

Mr. Stanley:—The energy on the lamp is exactly the same. f it were not the lamps would grow brighter. The potential on em is constant. The current going through them remains onstant or they would not be of the same brilliancy. The lf-induction coil does waste some energy, but it has the fect of storing energy for a brief interval of time and then re urning it upon the circuit. As to the invention of the self-in uction coil for this purpose, I have the very highest regard for rofessor Thomson and always had, but if I remember correctly, e patent that Mr. Rice refers to is not for this purpose. It is r cutting in a coil. This is a very different invention. This is coil always in circuit to do this work, while the coil Professor homson devised is cut into the circuit.

Mr. Mailloux.—I would like to state then that my conviction pears to be upheld by Mr. Stanley, that, the energy being the me, this induction coil represents nothing more than resistance. takes up a certain amount of energy which a certain idle re stance would clearly absorb. If we were to replace that self in uction coil by another one having no self-induction, but a larger sistance, so that the circuit going into that resistance would sorb the same number of watts, then we would evidently have e same energy absorbed as we have by that induction coil. So at I do not see the induction coil in question does anything ore than to replace a lamp that is put into that circuit. The se is exactly analogous to that of an electric motor that would be placed in circuit. The energy absorbed by an electric motor is the difference of potential into the current.

It is just exactly the same case in that induction coil. But in the case of the electric motor, as in the case of the self-induction coil that energy can be divided into two quantities, one of which is the C^2 R, the other of which is the product of the current into the counter-electromotive force. Now at first you might think that in using a motor as a resistance, the counter-electromotive force of the motor would act, so to speak, statically, upon the total electromotive force and reduce the total expenditure of energy in the circuit; but as long as the current is not reduced it can be readily seen that there is no reduction of the energy expended, because you simply have a certain amount of work which is absorbed electrically, and which that motor does either in useless friction, or in useful work, in connection with a Prony brake or some other piece of machinery. Therefore, in addition to C^2 R representing the necessary loss,—we have another quantity which is the product of the current into the counter-electromotive force exerted by the secondary coil, whatever that may be. Now what I would like to know, if possible, is how that can become dissipated. It certainly seems reasonable that if there is extra energy dissipated there that the coil ought to become a great deal warmer than if it were not doing any such work.

Mr. Wetzler.—It occurs to me that perhaps this phenomenon might be due to an error, or a peculiarity, of the measuring instrument employed. Mr. Stanley said that he employed a Cardew voltmeter. I would like to know whether he ever applied the electro-dynamometer for that purpose.

Mr. Stanley.—The electro-dynamometer is a very unfortunate instrument to use with an alternating current, as its reading is changed by a slight fluctuation in the alternations. The Cardew voltmeter, which is dependent on the amount of heat produced, is entirely independent of the alternations. Perhaps I can explain that. The coil is acting exactly as a dynamo would act in series and it chokes down the current in direct proportion as the resistance in this part of the circuit is raised. The average electromotive force at the terminals of the coil, however, is greater then would seem to be warranted by Ohm's law.

Mr. Howell.—Mr. Stanley said that inasmuch as the current was constant, as shown by the lamps; that is, the current in three lamps was the same as the current in four and that the electromotive force acting on the circuit was also constant, that the energy was constant in the circuit. I would infer then that $C E$ is the measure of energy in the alternating current. Is that a correct conclusion?

Mr. Stanley.—No, sir; that was a slip of the tongue perhaps on my part; but it is a most unreliable quantity. The energy of three is the same as the energy on the three lamps before. I think in reality there is very little waste of energy. But there is an apparent amount of energy wasted there—that is $C E$ of the coil. This is apparently as much as the energy expended on the lamp.

Mr. Howell.—I think that that diagram is a very good illustration of the fact that $C E$ is not a measure of the energy in an alternating circuit, because you have only a value for C there, and the value of the energy in that circuit would be $C \times 200$. Now that is the sum of the energies in three lamps and the coil, and in each lamp it would be $C \times 50$, and in the other it would be $C \times 100$; so that the sum of these energies is not the same as the sum of the whole circuit computed the other way, which shows that $C E$ is not the measure of the energy in the circuit.

I would like Mr. Stanley to demonstrate why the current in the secondary increases, or why an increase of current in the secondary increases the current in the primary.

Mr. Stanley.—I think that it is very easy to understand from the diagram, Fig. 7, of the counter-electromotive force, providing no current is flowing through the coil. The secondary electromotive force will of itself induce another electromotive force and so we might go on inducing forever electromotive force. We cannot merely transform energy once; we can transform it a thousand times. Now the secondary electromotive force will develop a counter-electromotive force on the primary circuit, and that will be still one-quarter of a period later. Notice that I have drawn these curves directly opposite. It exactly opposes the primary electromotive force at the same instant of time. Now, when the current is abstracted from the coil, this is no longer the position of the secondary electromotive force; it is shifted on. Consequently, the counter-electromotive force no longer opposes the primary electromotive force at the maximum time of the primary electromotive force, and consequently its value as a counter or opposing E. M. F. is very much lessened. You have asked me a difficult question.

MR. WHEELER:—While of course Mr. Stanley's explanation is exactly correct, I think that Mr. Howell's question could be answered in popular language by saying that the drawing of current from the secondary coil reacts on the electromotive force. In other words, drawing off a current from the secondary coil cuts down the magnetism and makes it harder for the primary circuit to magnetize the iron of the converter.

I would like to return to the three lamps and the induction coil, because I wish to ask this question:—Mr. Stanley stated, (see Fig. 9) that if this was connected into a circuit of two hundred volts, *e* would measure one hundred volts and that the lamps *f*, *g*, and *h*, would each measure 50 volts, making a total of 250. Now I would like to ask Mr. Stanley what he would get if he measured the potential between the conductor and the right-hand terminal of the lamp *b*.

MR. STANLEY:—I never have tried it.

MR. WHEELER:—I think that would be an interesting thing to find out; because there are 50 extra volts to be cornered somewhere, and I would like to know just where they are to be found.

MR. MAILLOUX:—I have been in the habit of reasoning in relation to secondary currents by analogy to the electric motor, and I consider it a perfectly logical and applicable method. In this case, as is well known, the electrical energy applied is the sum of two quantities, one of which is the familiar $C^2 R$, or energy expended in heating the motor, and the other (εC), the energy available for conversion or the product of the counter-electromotive force into the current. I will adopt the direct current system of reasoning because it is one which we all understand.

If, as Mr. Stanley says, there is but little difference in the amount of energy when the induction coil replaced the lamp, then it is evident that some of the energy must have gone somewhere.

The energy absorbed in the four lamps (Fig. 9) is evidently the square of the current that is passing through them, into the resistance, or $(C^2 R) = C^2 (r + r + r + r) = 4\, C^2 r$. To pass this current through the lamps a certain difference of potentials is necessary at their terminals; that is to say at the leads (+ –). In the continuous current system, we could therefore express in another way the energy absorbed in the lamps, and I have no doubt we could do so in an alternating system, if we can get the mean E. M. F. as is done with the Cardew voltmeter. We would then have for the expression of this energy, the product of the difference of potentials, into the current, ($C\ E$); consequently we can say :

$$\begin{aligned} C^2 R = 4\ C^2 r &= C\ E \\ &= C(e + e + e + e) \\ &= 4\ Ce. \end{aligned}$$

Now, when we cut off one lamp, and put a coil in its place, we still have the same potential difference at the terminals, that is evident. Since the other three lamps remain of the same brightness, they each have the same energy ($C\,e$) given to them as before. Observation showed moreover, that the potential difference of each lamp remained as before (e); hence, the current (C) must be the same. Consequently the energy sent into this branch must be the same as before, and the energy given to the coil must also be the same as that taken up by the lamp it replaced ;

$$4\ C^2 r - 3\ C^2 r = C^2 r$$

or to express it more conveniently,

$$4\ Ce - 3\ Ce = Ce,$$

the latter term being the same as the energy given to one lamp.

In the case of the lamp all of this energy was absorbed in heating resistance, but with a self-induction coil this cannot be so, for the resistance is very small. We must therefore, decompose the product ($C\,e$) into two other quantities. We now have a counter E. M. F. to deal with, produced by the self induction coil, just as in an electric motor, and which I may express by the same symbol (ε). Taking r' to indicate the much smaller resistance of the coil, we have

$$Ce = C^2 r' + \varepsilon C.$$

This is the energy given to the coil and as we see, only a part of it ($C^2 r'$) has the term resistance (r') in its expression and this portion disappears as heat in the coil itself. *The other portion* ($\varepsilon\ C$) *does not involve resistance and consequently does not disappear as heat in the coil.* It represents energy available for conversion, just exactly as it does in the case of a continuous current motor. This erratic energy ($\varepsilon\ C$) is the very energy which I wanted to send Mr. Stanley looking for. It is evident that it must have been taken up by or restored to the circuit. It occurred to me that it may have gone in some of the other branches. There are a lot of those lamps connected together. As I understand Mr. Stanley to say ; this is a multiple-arc connection.

MR. STANLEY :—All in series—every one of them.

Mr. Mailloux:—It would not make any difference for we have in both cases a certain time in which there is no current in the circuit, during which our induction coil will be charged. I am satisfied that the product ($\varepsilon\ C$), must go somewhere and it is going to do work either by heating the coil or wire, or else it is going to do work by producing E. M. F. in the circuit. Now the electromotive force which is due to this product, (which I designate by ε, as being a part of the energy that is being taken up by the coil), or in other words the counter-electromotive force—may act by producing energy at a time when there is no current, for, as we know, the wave of current in this branch would necessarily lag behind that in the others. Consequently, there would be a time when the original impulses passing through the circuit would reach zero, while this had not reached zero. I would like to ask Mr. Stanley whether those impulses which would be produced there, and having this reaction and this retardation—would not produce an impulse of their own through the circuit, which would account for the energy that must be dissipated somewhere or other.

Mr. Stanley:—I do not know where it goes. I presume the lag certainly does throw back the electromotive force to the rest of the circuit. It does not throw it out into the air. You wind a wire around a piece of iron, submit it to an alternating current; for all values below saturation it has this counter-electromotive force. That counter-electromotive force does not waste any energy in overcoming it. It simply reduces the amount of potential which is absolutely applied, to $C^2\ R$, but when the core is saturated in the coil, then the coil acts like a straight line. A very pretty illustration of the way the connection of the Gaulard and Gibbs apparatus in series—they connect converters in series and hitch them to a dynamo in which a definite quantity of current was always forced through the primary circuit. That amount of current was enough to nearly saturate the cores of the coils in order to obtain self-regulation. But if, through any misadventure the electromotive force raised a small percentage, the coils no longer opposed any counter-electromotive force and the current would follow through the coils at a most terrible rate, destroying the dynamo and producing all kinds of troubles. In this coil in the series arranged here it seems to me that this coil merely reduces the electromotive force and does not waste any energy as it simply passes the electromotive force that the dynamo applies.

Mr. Mailloux:—I do not think that it can be considered as acting merely to reduce. It is quite certain to my mind that it must act as a spring would when you compress it and when you release it, as you do at every alternation. That spring must return energy somewhere. A part of that energy is due to friction of the molecules in heating, but there is also a part which is available for work and that part must either return energy into the circuit or else it must go out into the air. I cannot for a moment entertain the idea that the induction coil in that place would simply act to reduce electromotive force, and I think if anybody stops to study the subject they will arrive at the same conclusion. I do not think it can be demonstrated either mathematically or in any other way that the induction coil acts in that manner.

Mr. Wheeler:—We were told a little while ago that there was no loss from retardation of induction with an alternating current. I would like to ask Mr. Stanley what the difference is between sending an alternating current over an electric light wire or an ocean cable.

Mr. Stanley:—I am not very familiar with ocean cables. But I presume the character of the current explains the whole difference. The character of the current bears a very important relation to the resistance which it possesses. If you have two circuits, ocean cable for one and ordinary overhead wire for another, the same number of alternations will not do for each; because, as you might say, the magnetic lag of the ocean cable is very much greater than it is from a short electric light wire. If you apply a proper alternation to an ocean cable, I should certainly expect to find no real loss in transmitting it. If, on the other hand, you apply it to an over-head circuit, in alternations so rapid that the circuit cannot receive it without impeding the on-coming impulses, you cannot transmit the alternating current over the over-head circuit without a loss.

Dr. Vander Weyde:—Could you charge a Leyden jar with an alternating current?

Mr. Stanley:—No, sir; I do not think so.

Mr. Wetzler:—It occurs to me to bring forward the point, namely: What we are measuring when we apply an instrument to an alternating current circuit? This curve Figure 20, represents the rise and fall of potential. We are measuring under ordinary circumstances, I should say the distance *e d*. May it not be possible that under certain conditions, we may be measuring, say, the difference between y and y'; this is a greater difference of potential, indicated as the electromotive force between the terminals of the alternating current circuit, depending on the

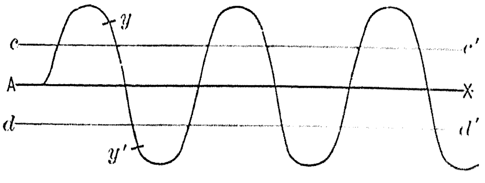

Fig. 20.

conditions under which the instrument is applied, or the organization of the circuits themselves.

Mr. Prescott:—As it is a well known fact that alternating currents do not follow Ohm's law, and nobody knows what law they do follow, why might we not expect the fact that the one hundred may be dealing with entirely different units than the other fifty—we do not know anything about it in fact.

The President:—I presume that there is no further discussion; and I think that I may safely say that of all discussions we have had before the Institute, we have had none more valuable, none more suggestive; and I think we shall all appreciate the fact that it has been of material service to us to have been able to be here this evening and take part in the discussion, or listen to what has been said.

I would like to announce that our next meeting will be held on December 20th, a special meeting, when a paper will be read by Mr. Arthur Steuart, the legal counsel of the Electric Light Association, on Reforms in the Patent Law. I may say that this matter of patent reform is one in which it is advisable not to move hastily, and it is a question that ought to receive the largest ventilation possible. In that view it is extremely desirable that the attendance should be as large as possible on the evening in question.

On motion the meeting adjourned.

The following letter, supplementing the foregoing discussion, has been received from Mr. Rice.

Lynn, Mass., Dec. 7th, 1887.

T. C. Martin, Pres't.

Dear Sir:—I regret greatly that it was necessary for me to leave before the interesting discussion on the subject of alternating currents was concluded, as I was obliged to take the train for Lynn.

I was anxious to state in answer to Mr. Stanley that I had no desire to disparage the work of Mr. Shallenberger, as I have the highest regard for his genius as an inventor and for his ability as an electrician.

The point which I desired to emphasize was that Prof. Thomson was the first person to proportionate a coil so as to act as a substitute for a lamp, and also the first to publish the same, in the patent referred to. It is true that in Mr. Shallenberger's arrangement the lamp is shunted around the reactive coil, while Prof. Thomson shows the reactive coil designed to be switched in place of the lamp, this latter method being preferable on the ground of economy, as when the lamp is shunted around the coil a certain percentage of the energy is wasted by the coil.

I was astonished to hear Mr. Stanley make the statement that the energy used was the same in the two cases of four lamps, of 50 volts each placed in series across alternating mains, with a difference of potential of 200 volts, and that of a reactive coil substituted for one of the lamps. It may be interesting to mention an experiment which Prof. Thomson made in my presence several years ago, and which demonstrates very beautifully the fact that a reactive coil can be substituted in place of a lamp with but very little absorption of energy. A constant current was reversed in direction by means of a commutator through the primary circuit of an induction coil, and the secondary circuit of the coil was used to supply about six incandescent lamps, Fig. 21. Properly proportioned reactive coils were arranged to be substituted for

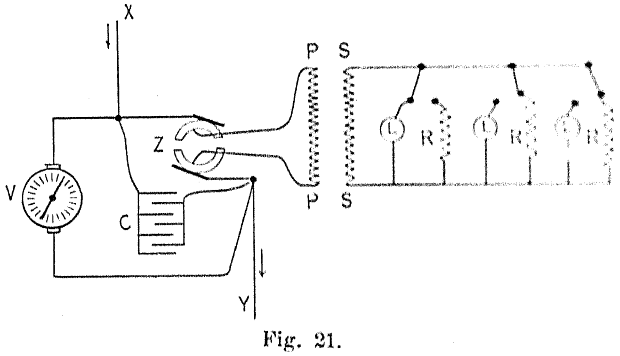

Fig. 21.

the lamps. It was found that by shunting the terminals of the primary circuit by means of a volt-meter placed beyond the commutator that the potential was greater when the lamps were in action than when the reactive coil substitutes were put in place of the lamps. As the current was constant, about 10 amperes, this demonstrated that the coils were absorbing but little energy. It was found that as the coils were substituted in place of the lamps that the brilliancy of the remaining lamps did not change. This was true even after five of the lamps had been replaced by reactive coils, the remaining lamp having the same brilliancy as before.

The same fact is also demonstrated by another experiment which was tried about the same time, (Fig. 22.) In this case a motor generator, so-called, was used, actuated by a continuous constant current from an arc light line, the generating portion producing alternating currents. Lamps were shunted across the terminals of the alternating current generator and the potential existing between the motor terminals noted when the generator was loaded with about 10 lamps. Reactive coil substitutes were now put in place of the 10 lamps, and it was found necessary to shunt the motor field by a resistance in order to reduce the potential between the motor armature terminals, otherwise its speed would rapidly increase as the reactive coils were substituted in place of the lamps. This indicated that less electric energy was required by the motor when reactive coil substitutes were used in place of lamps.

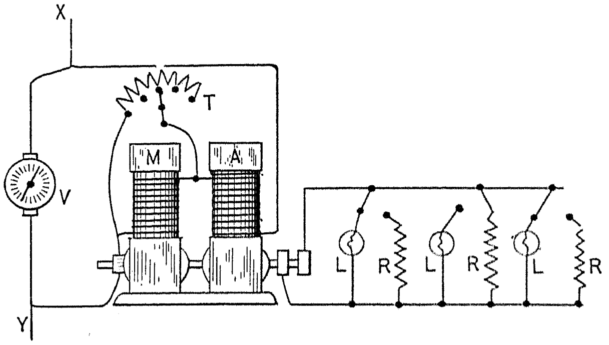

Fig. 22.

It was found that, as in the preceding case, the brilliancy of one lamp when running in connection with nine reactive coil substitutes was the same as when running with the other nine lamps.

Yours very truly,

E. W. Rice, Jr.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. V. New York, February, 1888. No. 5.

SPECIAL MEETING.

Held at the House of the American Society of Civil Engineers, 127 East 23rd Street, New York, December 20, 1887.

The meeting was called to order at 8 o'clock, P. M., by the President, Mr. T. C. Martin.

THE PRESIDENT:—I am very glad that, in spite of the inclemency of the weather, we have so large an attendance this evening of those who are genuinely interested in this question of patent reform. The subject of the revision of the patent law will be brought before us this evening by Mr. Arthur Steuart, who is the general legal counsel of the National Electric Light Association. The subject is one, I may say, that lies well within the province and jurisdiction of the American Institute of Electrical Engineers. The subject has been brought before us on various occasions; and it is somewhat interesting to recall the fact, that at our very first meeting held in this room for purposes of organization, now close upon four years ago, almost the first question that was introduced to our notice, was that of Patent Reform, when Mr. C. J. Kintner, of the Patent Office, presented a spirited appeal to us for aid in bringing about certain reforms which he then deemed necessary. Since that time we have had valuable and forcible papers presented before the Institute by Mr. Frankland Jannus and by Mr. A. C. Fowler, both of whom brought out very ably the necessities of the situation at the time. It will thus be seen that the Institute is well prepared to consider the subject in all its bearings and relations, and is ready to ascertain from Mr. Steuart just what work the National Electric Light Association now proposes to do, with regard to this bill to be introduced before Congress. I may say here that I have myself had the opportunity of attending all the meetings of the National Electric Light Association, at which this question has been brought for ward, and I think I may take the liberty of saying that that society does not intend to act with any undue haste or precipitancy in the matter, but is likely, in fact is sure, to move with its accustomed practicability and business-like common sense. Our position to-night is one in which we may be able to do great good in the direction of showing how far this movement is calculated to receive the support and sympathy of scientific and professional circles, especially those that are connected with electrical interests. I have much pleasure, therefore, in asking Mr. Arthur Steuart to read us his paper on Revision of the Patent Law.

REVISION OF THE PATENT LAW.

BY ARTHUR STEUART.

I come before you this evening at the invitation of your Secretary, for the purpose of telling you of the effort that is being made by the National Electric Light Association, to secure some reforms in the Patent laws of this country and of attempting to get you to join hands with us in carrying these reforms into effect. I particularly desire that the suggestions I shall make may be fully discussed by those present, for I hope to gain many valuable suggestions from you. For the purpose, therefore, of presenting what I have to say in the shortest possible time, so as to leave as much as possible for other gentlemen, I have condensed my suggestions and shall state them with very little argument; first, for lack of time, and secondly, that I think most of the reforms suggested are so apparent that they will call forth little opposition. But before proceeding to discuss the details of the law that require changes and additions, I cannot refrain from making a few remarks upon the patent system in general and its influence upon the industrial development of our country. Who can measure the benefits that this land has derived from the inventions that have been produced by its citizens. The roll of honor is too long to be repeated. The cotton gin, the printing press, the reaper, the application of steam to all the uses of civilization, and latest but not least, the modern application of electricity in its multitudinous forms; but while these brilliant achievements occupy our attention, we must not overlook the thousands upon thousands of inventions relating to matters of detail in every industry that have cheapened the product by saving time, material, power or labor. The fact is apparent that our advancement as a manufacturing nation has been due to the introduction of new methods of work which have enabled us to produce staples at such a price as to compete with other nations in the markets of the world, or in the development of new devices which have supplanted old ones or created a market for themselves. This being true, we must look for a cause for this development. What has induced the people of this country to create so many new things, to labor so long and arduously to lessen the cost of production of some article, or to supply a new one to take its place which is better or cheaper?

I think we may state it as a proposition that will not meet with denial, that the prevailing motive of the American people is a motive of pecuniary gain, a desire to amass wealth, to attain power by means of money, and whatever avenue offers them this reward will be the one into which they will press with a vigor and determination that knows no such thing as failure.

When the framers of our Constitution inserted into it the clause which provides that "Congress shall have power to promote the progress of science and the useful arts, by securing * * for limited times to * * * inventors the exclusive right to their * * * discoveries," they laid a foundation stone upon which an edifice has been constructed, that has far surpassed anything they had conceived. Upon this provision of the Constitution is our Patent Law based; from it, it takes its spirit; and with the view of adhering more closely to this spirit, have the amendments to the Patent Law been suggested, to which I shall ask your attention this evening.

The patent laws of the United States, passed in pursuance of this provision of the Constitution, open up before every citizen of this country, as well as before those of many others, opportunities of obtaining wealth and power that are not presented by any other set of circumstances under the sun. In a single moment of time, by a quick operation of the mind, the poor mechanic may be put into possession of an idea that, by its own development, may place him among the princes of the land. This is the possibility that presents itself to every man, rich or poor, high or low, who comes to understand the meaning of our patent system and its results. What force can be compared with this, to produce in the minds of men, thought, deep, concentrated and continuous, for the production of inventions, a thirst for knowledge, and a frugality of life that will supply a surplus of funds for the acquisition of knowledge and the development of inventions, and an ambition for their children that they shall have greater advantages of education upon which to work than their parents. And who can estimate the value to a state of such influence upon its citizens. This training, foreign manufacturers testify, produces in American operatives a power of comprehending their work, and a rapidity and dexterity of workmanship, that makes it impossible for them, with labor at one-half the cost and their factories fitted with the latest American machines, to produce the same goods as cheaply as they can be produced in this country.

It therefore goes without saying, that with such inestimable good to the nation, both in improving its industries and its people, to be attained by stimulating inventors to their highest effort, that the only true course for the law-makers to pursue in framing legislation relative to this subject, is to endeavor to make the process of obtaining a patent upon an invention as quick, easy and inexpensive as possible, and after it is granted to throw around it, in the hands of its owner, every form of protection known to the law, that will enable him to realize the full intent of the Constitution and enjoy the exclusive right to make, use and vend the invention.

Our patent system, as it exists to-day, is a grand structure, but it has some defects, which are due principally to the fact, that the demands made upon it by the development of our industries have far exceeded anything that the framers of the present law contemplated.

The Patent Law of to-day is substantially identical with the law passed by Congress in 1836. This law was revised in 1870, and codified in 1874; but few changes were made in it, and little, if any, provision was made, to accommodate the immense mass of business that has been cast upon the Patent Office; and some of the new features that were introduced have been found to work a hardship upon inventors rather than a benefit; so that practically we are working under a law formulated fifty years ago, when the work of the Patent Office for the whole year was something less than it is to-day for each week. Is it surprising, therefore, that the child should have outgrown his clothes? And this is practically just what has taken place. A commissioner and a half-dozen examiners were quite equal to the task of examining intelligently four hundred and fifty patents a year; but the case is a very different one, when the Commissioner has under him something like two hundred examiners, and when the annual issue of the office is something over 25,000 patents.

If time permitted, I could show you how utterly impossible it is, for the work of the Patent Office to be accomplished, with any degree of satisfaction to the public, under the prevailing organization; but I know that most of you are quite as well aware as I am, of the defects that exist and are quite as much interested in removing them. I shall pass at once to the consideration of the measures of reform that seem desirable in the Patent Office.

I. THE PATENT OFFICE SHOULD HAVE THE EXCLUSIVE USE OF ITS BUILDING.

All those who have had any experience with the internal operations of the Patent Office are aware of the fact that this office is greatly cramped for space. The examiners are often crowded into rooms that are so small that there is scarcely space enough between the desks to move about. Many of the officers and employees are compelled to work in quarters that are unfit for human habitation; simply because there is no other place to put them; and the documents and records of the office are so crowded, and the places in which they are kept are so unfit for their accomodation that they are in constant danger of destruction. The Patent Office building was constructed with money, every dollar of which was paid to the Government by patentees and applicants for patents, and yet the Patent Office is crowded into less than half of this building. The Interior Department, with its Land Office and Indian Department, occupy the balance and are constantly encroaching.

The Interior Department should have separate quarters and the Patent Office should have the whole of the building which will not more than accommodate it.

II. THE PATENT OFFICE SHOULD HAVE THE WHOLE OF ITS OWN INCOME.

For the past fifty years, the Patent Office has paid all of its own expenses, has built its own building, and in addition to this, has paid the Government an annual revenue, which aggregates to-day, without interest, something over three millions of dollars. And yet the most difficult task that the Commissioners of Patents have had to perform, has been to induce Congress to authorize the expenditure of a part of this revenue for the necessary expenses of the Patent Office. For the proper dispatch of the work of the Patent Office, considerable additional force is necessary. The Commissioner of Patents should be authorized to expend the whole of the income of the office for its own expenses, and what was not spent should be set aside as a fund to be used for the benefit of the office at any time that it is needed.

III. EXAMINERS.

The staff of examiners in the Patent Office have charge of public interests of a most vital nature. They are the virtual judges of whether an inventor shall have a patent for his invention at all, and also of what kind of a patent he shall have in case they give him any.

It is therefore apparent that in order that they may perform their duties with intelligence and efficiency they must fulfil several conditions.

1st. They must be thoroughly educated in the technology of the arts and sciences, and in the Patent Law. Experience has shown that this training can only he acquired in one or two ways,—either by systematic teaching or by years of experience in the practical work of the Patent Office; the latter method is the one now in operation and it results in creating a class of Examiners who are undergoing the process of education at the expense of the unfortunate inventor whose case may fall into their hands. The inventors of the country have a right to demand that this system of education of examiners at their expense be discontinued and a system adopted that will secure to the Patent Office a corps of examiners who will be thoroughly trained before they enter the office, and to the inventor the examination of his case by a man fully equipped to make such examination in the best manner.

2nd. Having obtained a staff of competent and thoroughly equipped examiners, they must be retained in office, so that the public may have the benefit of their services through a number of years. The Army and Navy have long since solved this problem by the exercise of a system of education, compensation, promotion and retirement on pay, which fully secures these ends. Why should not that system, so successful in one branch of the Government service, be established to secure similar and equally necessary results in the Patent Office?

3rd. The examiners should be relieved from all political influence. Civil service reform has attacked the question of political interference with Patent Office appointments with little success. The evils from this cause now existing are great and call loudly for remedy. The Army and Navy system put those departments of the public service almost entirely beyond the reach of the politicians. Thus it should be with the Patent Office.

IV. SALARY OF COMMISSIONER OF PATENTS.

The salary of the Commissioner of Patents should be very high, so that every examiner may feel that if he is faithful and does his duty, he may some day, in the natural order of promotion, reach a place of honor and large emoluments. This is the system of the Army and Navy, and in those departments it works admirably.

V. A PATENT COURT SHOULD BE ESTABLISHED.

The greatest evil that exists to-day in the administration of the patent system, lies in the lack of harmony between the Patent Office and the Courts of the United States, in which infringement cases are determined. Every practitioner before the Patent Office knows how little regard is paid by the Examiners to the decisions of the courts upon questions of Patent Law, and it has long since become a thing of the past, when the courts give any weight to the presumption of patentability, that would naturally arise from the fact of the Commissioner of Patents having granted a patent upon a device. For the accomplishment of the best results with our patent system, the interpretations and application of the law under which patents are granted should be the same as that under which rights growing out of patents are determined. So that a right granted by the Patent Office, would be sustained by the Courts, and the rulings of the courts would be in conformity with the spirit of the Constitution which has originated the whole system.

Under the existing system the examiners in the Patent Office interpret the Patent Law for themselves; and it has been my experience that few of them agree upon many points. The practical questions, which have to be passed upon by the examiners, seldom, if ever, get before the Circuit Courts, but are decided by the Commissioners of Patents, who it is well known, seldom possess any knowledge of Patent Law when they enter upon the duties of the office, and seldom occupy the position long enough, to be of any practical service to the office after they have learned something of the law by experience. The result is, that a majority of the cases passed upon by the commissioners and which are intended for the guidance of the examiners, are decided at a time when the particular Commissioner is almost entirely ignorant of both the law and the practice of the office. It is, therefore, only to be expected that the examiners should form their own notions of the law and pay little regard to either Court or Commissioner, and as the examiners are not lawyers and have no knowledge of the Patent Law, except such as they may gain from the conflicting decisions of the ever changing commissioners; and as they have had no judicial training, it follows as a matter of course that each primary examiner has his own interpretation of the law and his own peculiar notions of how it should be applied to the practical questions that arise in his daily work. The result is that we have as many different patent offices as we have divisions, and scarcely any two of them with the same views of the law, but each having the power, and exercising it, of issuing patents according to his own individual notion of what the patent should contain and how it should be framed, and these patents thus issued are the title papers upon which the inventors of the country are compelled to base their claim, when they come into the courts for the purpose of protecting their property. Is there any wonder that it is difficult to obtain a patent of any value, or to sustain and protect it after it is procured? It seems, therefore, to be eminently desirable that the same tribunal (which shall be so framed as to insure its capacity) should have appellate jurisdiction from both the Patent Office and the Circuit Courts, so that by vesting both jurisdictions in one competent court, the rulings of both branches, the Patent Office and the Circuit Courts, may be made uniform and the anomaly removed that now exists of one branch of the Government employed in granting patents, and another, in destroying them; it may be said that such a Court exists at this time, but we all know how far removed the Supreme Court of the United States is from the Patent Office and how little influence it has upon its practice; beside which, the existing evils have grown up under the present system which demonstrates its incapacity to remedy them. A strong, well selected Court of expert patent lawyers would have many advantages. It would harmonize the Patent Office and the Circuit Courts. It would crystallize the practice of the Patent Office, so that it would be possible for attorneys to form some conception of what that practice is and be able to advise their clients with safety with reference to their rights. The inventor could have some hope that when he had complied with his part of the government contract, by disclosing his secret, he would be protected in the exclusive right to use it. Such a court would lift a great burden from the shoulders of the Supreme Court, as but a small proportion of cases decided by it would be appealed to that court, and its rulings being binding upon the circuit courts would enormously simplify the practice in those courts.

The jurisdiction of such a court should be altogether appellate and should consist in appeals from the Board of Examiners-in-Chief upon question of anticipation, patentability and practice, from the Examiners of Interference, upon questions of priority between applicants for patent upon the same invention, and of appeals from the circuit courts of the United States upon all questions relating to patents that now go to the Supreme Court. It would be proper also to include, within the jurisdiction of this court, all questions now passed upon by the Commissioner of Patents, such as trade-marks and labels; copy-rights might also be included. With such a court the duties of the Commissioner of Patents should be purely executive and the jurisdiction of the Supreme Court of the District of Columbia in matters of appeal from the Commissioner, should be cut off. The Supreme Court of the District, should bear the same relation to this court as the circuit courts.

Passing from the question of the reorganization of the Patent Office to specific amendments to the law, the first question that presents itself is the question of limitation to actions.

VI. LIMITATIONS.

It is pretty well settled by the courts that, since the adoption of the Revised Statutes 1874, there has been no limitation to the right of an owner of a patent to bring suit against an infringer for the recovery of damages for such infringement. Prior to that time, the only limitation that existed was six years after the expiration of the patent—and this seems to be the just rule—many rules for limitation have been suggested, but it seems to me, in view of the fact which all of us have experienced, that the last few years of a patent are often the only ones during which the inventor realizes anything from his invention, that to cut him off from the recovery of damages from infringers, who may have pirated his invention during a period when he was too poor to stop them, would be unjust in the extreme.

VII. DAMAGES.

The existing equity rules, for the recovery of profits and damages for the infringement of a patent, are in very satisfactory shape; but since the decision of the Supreme Court in Root *vs.* The Railroad Company, these rules do not apply to actions brought for the recovery of damages after the expiration of the patent. Such suits must be brought at law, by an action of trespass on the case, and the measure of damages under that form of action are much less liberal than the rule in equity. In an action of trespass on the case, the measure of damages is the actual loss to the plaintiff and not the gain to the defendant; while in equity the rule allows the recovery of either, loss to the plaintiff or the gain of the infringer, whichever may be the greater, and often the difference is very great. If it is just and right that the owner of a patent should have all the benefits that may or can be derived from the use of his invention, and this is the theory of the law, and in one form of action before the expiration of his patent is allowed to recover these benefits, in the absence of a statute of limitations, why, in justice should he be deprived of this benefit simply by the expiration of his patent? It would seem as if this evil should be remedied by legislation. It might be done by giving a plaintiff a standing in equity, under a bill for an account and discovery independent of an injunction; or if the patentee were permitted to bring an action of assumpsit, for recovery of the benefit that the infringer had derived by the use of his invention, this might be almost as well; but this would require the interposition of a jury, and in complicated patent cases it is difficult to accomplish satisfactory results with a jury. It is almost impossible to make a jury understand mechanical technicalities, or for the attorneys to do justice to the case upon oral testimony.

We have at this time a case in point which illustrates this rule. For seven years the owners of certain patents have been in constant litigation, seeking to sustain them. We have recently succeeded in getting a decree in their favor, and a month before this decree was rendered the patents expired. The patents have been extensively infringed and cover important inventions. We have now no remedy save to sue at law by an action of trespass on the case. As a matter of fact the owners of the patents have never established a license fee for their use, but if they had, as they might have done, it would not probably, have been more than $10, for each machine, hence this sum in our action at law would have been the limit of our possible recovery; notwithstanding the fact that the infringer by the use of each machine has saved to himself no less than $500. Why should the rule of law in a case like this be different from that, in a case where a man without my knowledge or against my will carries off my horse and uses him for his own benefit for a certain period, after which I recover him. In such a case, I bring a suit and the jury estimate the benefit that the wrong doer has derived from the use of my horse and award me this amount as damages for his wrongful act. Would it be right that this wrong doer should have the power of escaping liability, by answering when the jury found that he had made $100 by the use of my horse, that I could not in the same time have made but $10 with him? Would not this be putting a premium upon the wrongful act?

The question is still an open one, whether in those states where the common law forms of action have been abolished, an action could be maintained at law for the infringement of a patent the substantial result of which would be the same as the common law action of assumpsit, although the statute prescribes an action of trespass on the case as the proper action.

VIII. REISSUES.

Since the decisions of the Supreme Court in Miller *vs.* The Bridgeport Brass Company and the line of cases that have confirmed this doctrine, it may be said that a reissue in the sense in which that term was formerly applied, is no longer possible. The unfortunate inventor who has employed in his ignorance, an incompetent solicitor to procure his patent, or who yields to the unnecessary limitations of the office, and whose patent issues to him covering half of his invention, or possibly contains a disclaimer disclaiming the other half, can be said to-day to have no remedy for his misfortune; and if two years have elapsed since he procured his patent, or some enterprising manufacturer examines his patent, and finding that he has not covered the second half of his invention, proceeds to make and sell that half, he is for ever barred from going back and claiming it. Some change in the law should surely be made, to enable an innocent and meritorious inventor to secure the full benefit of his invention, if this can be done without destroying vested property rights in individuals.

Under the present law, when a patent is surrendered in order to obtain a reissue, all accrued causes of action for past infringement upon the original patent go with it, and no action can be brought for an infringement committed prior to the date of the reissue. There does not appear to be any good reason for the existence of this rule.

At present an application for a reissue must be signed and sworn to by the inventor. This rule puts it into the power of a dishonest person to extract money from the owner of the patent by refusing to sign the reissue-papers. The owner of a patent should have the power to deal with his property as he chooses, and as he is considered, when he purchased the patent, to have purchased all that could have been covered by the inventor's original application, if the patent does not do this, he should have the power to reissue his patent upon his own petition, and an oath that he is the sole owner, or if there be several, that they own the whole title.

IX. REPEAL OF PATENTS.

The statutes of the United States contains no provision which will permit a patent, supposed to have been obtained by fraud or mistake, to be set aside, nor for any action by the United States against the owner of such a patent to prove the fraud and have the patent declared void. The statute should contain such a provision.

X. EQUITY PROCEEDINGS FOR SLANDER TO PATENT.

Neither statute nor precedent in this country authorizes an equity proceeding for an injunction to restrain the continuance of slander against a machine or a patent, by one who falsely charges that the machine or the patent is an infringement of another patent owned by him, and threatens suit for such infringement, but neglects to bring such suit, and who thus continues to disturb the business of the party slandered, without attempting to execute his threats or to vindicate his claim. Such an injury cannot adequately be remedied by an action at law for damages; what is wanted is an injunction from a Court of Equity to stop the tongue of the slanderer at once and forever.

XI. LICENSES.

Licenses should be recorded and subject to the same rules as mortgages of land, the license first recorded should take precedence.

XII. JOINT OWNERS.

The mutual rights of joint owners of a patent are in a very unsettled state in the present condition of the law, and it would, I think, be much safer to say that joint owners are entirely independent of one another, although there are some cases to the contrary, than to venture any statement, with reference to what those mutual rights may be.

It would seem that no good reason could be advanced why joint owners of a patent should not bear the same relation to one another as joint owners of any other property. If two persons own a house jointly, but in definite proportions, and one of those persons rents the house and receives the rent for it, the other can of course receive his share of the rent either from the person or from the property. The absence of well settled rules upon this subject makes it very desirable that they should be established by statute.

XIII. UNITED STATES PATENT LIMITED BY FOREIGN PATENT.

United States patents should not be limited to expire with the shortest lived foreign patent upon the same invention for the simple reason that the American inventor who gives his invention as a free gift to the foreigner, gets a patent here for seventeen years, while he who seeks to protect himself abroad may limit his United States patent by several years.

XIV. ASSIGNMENTS.

Assignments of patents should stand upon the same footing as conveyances of land, the assignment first recorded should take precedence.

XV. EXPENSE OF LITIGATION.

Expense of the trial of patent cases might be lessened if the Court was authorized, in impannelling a jury as provided by the act of 1875, to impannel a jury of five experts in the subject matter of the case, pay them a suitable per diem, let them hear the testimony, and advise the Court.

XVI. INTERNATIONAL CONVENTION.

The United States, by its adherence to the International Convention, pledged itself to adopt legislation as soon as possible to harmonize the existing law of this country with the international law. The practical result that Americans need in relation to foreign patents is a period of say six months, after they have secured an allowance of their United States patent, to make their applications abroad without affecting the term of their United States patent.

I have thus rapidly touched upon the points in our Patent Law that need revision, without attempting to formulate specific additions to the statutes. This, I think, can only be done effectually by a commission of expert lawyers, such as is contemplated by the bill that we propose to introduce into Congress. In order to secure the passage of that bill, we will need the aid of every man who is interested in this subject exerted to the fullest extent, we would most earnestly urge the importance of this effort in its possible effects upon the country, and solicit your assistance in making it successful.

DISCUSSION.

THE PRESIDENT:—Mr. Steuart's weighty paper is now before us for discussion. On such an occasion as this, when we have so many views to be represented, we shall be very glad to have the participation in the discussion of gentlemen who are interested, but who may not be members of the Institute, and before we proceed, I think I express the general wish when I call upon Mr. George Ticknor Curtis to open the discussion for us. Mr. Curtis is known to all of us as a foremost student and exponent of patent law in this country, and it would be of the greatest pleasure to us all to learn from him what his views are of the subject in general, and what are the specific points to which he considers it best for us to direct our attention in seeking any reform of the system. Mr. Curtis, we should be very happy indeed if you would favor us.

MR. CURTIS:—Mr. Chairman, I have listened with the greatest interest to the very able and comprehensive paper which has been read. It is a subject that has occupied my thoughts and exertions for a good many years. A year ago, last winter, I caused to be introduced into the House of Representatives a bill which I drew for the appointment of a commission having the same general object and purpose as the bill which you contemplate. The especial feature was a little different. I did it on my own responsibility, but I did not do it without consulting others. I consulted the Commissioner of Patents. I consulted one or two of the Judges of the Supreme Court of the United States and other persons. The bill was referred to the House Committee, and they gave a hearing on it to me, and other gentlemen. The Commissioner of Patents attended, and he urged more especially in regard to his own department the importance of the bill, and had a great deal to say, especially on the subject of the establishment within the Patent Office, of a special tribunal, such as has been sketched in the paper that has been read this evening. The bill met with great favor with the House Committee, and they unanimously approved it, after making some modifications in a few of its details. They changed some of the provisions in respect to the compensation of the Commissioners and provided a suitable compensation for a secretary, and they made a distinct appropriation, I think, of the sum of forty thousand dollars to cover the entire expense. The bill was reported to the House, but it was overslaughed with thousands of other bills that never passed that Congress. I happened that winter to have a good many cases before the Committee, as counsel or associate counsel, and I became very intimately acquainted with the different members of the Committee from different parts of the country, and I am convinced that if this association, and the other body that has been referred to, which I cannot describe by its proper title, shall take up this subject and resolutely carry it before Congress in the mode in which you contemplate, that you can get the bill passed. That bill which I had some instrumentality in preparing fell to the ground, of course, with the expiration of that Congress. I had a conversation with Senator Platt, of Connecticut, who is the chairman of the Senate Committee on Patents, and he raised this objection, that however good a code or bill might be, prepared by such a commission, that when it came to be enacted, it would be loaded down with amendments that would kill it. Well, my answer to that was, that would depend on the ability of the Commissioners to make a consistent and harmonious code, and one that would address itself on its own merits to the general legislative mind, and that it would also depend upon the ability of the Commission to explain the changes which they contemplated, or the improvements which they contemplated, in a report to accompany the bill; and I thought that if that could be done a majority of both houses would see the wisdom of passing it as it was presented by the Commission, and not undertake to amend it, but let it go into operation and be amended as the progress of time should show amendments to be necessary. This matter was never before the Senate Committee. This was simply an informal and private conversation which I had with the Chairman. Now, sir, it would take me half the night to touch all the points, or half of them, on which it seems to me the time has come for this kind of action. My situation and occupation of late years have led me to bestow a great deal of attention to the course of the Supreme Court in the administration of the Patent Law. Unfortunately—I do not know how it has come about exactly,—but the present court has gone to very great lengths in overturning all the great doctrines of the Patent Law that were settled forty years ago, when I first began to practice—and that is more than forty years ago. There have been decisions, which of course must stand as the law, until Congress shall interfere and declare the law otherwise. Well, that is one great part of the task which such a Commission would have to perform. They would have to examine all these cases and see whether they are, or are not consistent with the fundamental principles of the law as it has been construed heretofore. The statute law is substantially the same to-day as it was, when enacted in 1833. There has not been any very material change. But there is a very great departure in the doctrines that are held now by the judges of the Supreme Court, from what seemed to me to be the sound and true doctrines of the law, and I hope you gentlemen will go ahead and get your bill passed, and then secure as suitable a commission as you can. I have a copy of the bill of the last session, which I will hand to the President.* I suppose that your bill,

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*IN THE HOUSE OF REPRESENTATIVES, MARCH 24, 1884. MR. GREENLEAF introduced the following bill:

To provide for the revision, improvement, and amendment of the laws relating to patents for useful inventions.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the President is hereby requested and authorized to appoint three commissioners, one of whom at least shall be a person learned in the law of patents for useful inventions, and a practitioner of ten years standing in the courts of the United States, whose duty it shall be to examine, revise, and report upon the laws of the United States relating to patents for useful inventions, the remedies for infringements of such patents, and the internal administration of the Patent Office, and to report such amendments of the system as they may deem expedient, in a single code or in draughts of separate laws.

SEC. 2. That the said commissioners shall make their report within six days of the first day of the next session of Congress after the passage of this act.

SEC. 3. That the said commissioners shall each receive a compensation of five thousand dollars for their services, and an allowance of eight dollars per day for their necessary traveling expenses when actually engaged in the business of the commission away from their respective residences. They which in some respects is a better one, will go at once to the House Committee; and of all suggestions I should respectfully recommend that you avoid the appearance of aiming at any particular point, any particular change, in the present system of Patent Law more than others. The paper goes over a great deal of ground, specifies a great many different points. There would be differences of opinion, of course, about these different matters. With a great deal of it I agree. I do not know whether I should agree with every part of it, but if you can present your application in such a way as not to allow the Committee to have the idea that you have any axe to grind for yourselves, or anybody else, in respect to any specific question, why, I think there is no doubt but what you will succeed in carrying the measure that you wish to.

The President:—It has been very interesting indeed to have these remarks from a gentleman so well known, and whose experience extends over a period of well nigh half a century. In listening to such mature advice, and in governing ourselves by it, we certainly can not go very far amiss. Mr. Curtis, in his remarks, made allusion to an interview that he had had with Senator Platt. I may state that a few days ago I wrote myself to Senator Platt, calling his attention to this movement which had been set on foot by the National Electric Light Association, and which we proposed to bring within our purview, and I asked his opinion of it, saying that we would be very glad, if possible, to have his attendance at the meeting. I would like to read his reply. It is as follows:

"U. S. Senate, Washington, Dec. 17, 1887.

"T. C. Martin, Esq., President American Institute Electric
"Engineers, New York.

"Dear Sir:—I have your letter of the 15th, expressing your desire that I be present at your meeting next Tuesday evening in New York. It will be impossible for me to leave Washington at that time.

"With regard to the proposed revision or amendment of our

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shall be allowed to appoint a secretary, who shall receive a compensation of one thousand two hundred dollars for his services.

Sec. 4. That the sum of twenty-five thousand dollars is hereby appropriated to pay the expenses of the said commission, including the compensation and traveling expenses of the commissioners, the compensation of their secretary, and their expenses of printing, such payments to be made by the Secretary of the Treasury, upon vouchers certified by the commissioners, at such times as they may present such vouchers.

Sec. 5. That this act shall take effect from and after its passage.

Patent Law, I can only say, generally, that while I think some amendments are very desirable, it is very doubtful, first, whether such amendments as are desirable, can be obtained at the hands of Congress; and, second, whether any attempt to radically amend our Patent laws would not result in the passage of laws calculated to injure the interests of inventors and the public. I say this, because my observation leads me to believe that probably two-thirds of the members of Congress are more or less prejudiced against patents and against certain desirable features of the patent system, and during all my experience in the Senate, it has been my effort to prevent Congress from passing laws which would be disastrous. No man can predict the outcome of an effort to amend the Patent Law. It may result in what we deem the advantage of the public. It is quite as likely to result injuriously to the public. I have been made to feel very many times that perhaps it was better to bear the ills we have than to fly to others we know not of. In saying this, I do not mean to imply that I am not in favor of amendments, but simply to indicate the danger that always confronts us when we attempt to legislate with reference to patents.

"Very truly yours,
(Signed.) "O. H. Platt."

We have a representative of Connecticut with us in person this evening. I refer to Mr. Albert H. Walker, and his views on this subject would be likely to interest us very greatly.

Mr. Walker:—Mr. Chairman and gentlemen: I have listened with great pleasure, as we all have, to him whom we regard as the father of us all; speaking of ourselves as patent lawyers, or as interested in the development of the useful arts. The pamphlet that I see in the hands of some of you, and which, I suppose, has been read by most of you, contains an article by myself, which sets forth the principal part of my views touching the desirability of revising the Patent laws. It will therefore not be expected that I will elaborate my opinions upon that subject on this occasion; but I shall be glad, with your permission, to make some observations touching the expediency of pressing Congress to take the proposed step toward revising those laws. I do not agree in the slightest with Senator Platt. My experience in the attempts that have been made to secure from Congress the revision of the Patent laws, dates back to a period considerably earlier than that of Senator Platt, and with all deference to him, I think it has been more extensive than his has been, because during his occupancy of a seat in the Senate, no such effort of any degree of seriousness has ever been made. Ten years ago the celebrated bill, 300, was before Congress—the 45th Congress. Its forerunner was before Congress eleven years ago—the 44th Congress—and very strenuous effort was made to pass that bill. Very large amounts of money were expended in employing distinguished lawyers to apply to Congress and secure the passage of that bill. It was not favorable to the patent system as a science. It was advocated by men who had axes to grind. It was advocated by men of very great ability who had clients who contemplated plucking certain prospective prey of theirs, and it failed, because it was a bad bill: and I think that a bad bill upon the subject of the Patent laws can be relied upon to fail in the American Congress. A great many have failed and I think they all will fail. Why? Because there are so many intelligent men in Congress, and because there are so many intelligent men in the United States who can explain things to the intelligent men who are in Congress. If we are ever going to make an effort to secure a revision of the Patent Laws, no time is more opportune than this. I read this afternoon, on my way here from Hartford, an editorial published in the December number of the *Electrical Engineer*, taking the view of this subject which is taken by Senator Platt in his letter. The editor indicated in that editorial his opinion that a Congress, which sits pending a presidential campaign and pending the counting of an electoral vote, is a very inopportune Congress to which to apply for a revision of the Patent Laws. But half of our Congresses come within that category. Half of our Congresses sit in the last halves of presidential administrations, and the other half in the first halves of presidential administrations, and those Congresses which sit in the last halves of presidential administrations are generally understood to attend chiefly to presidential elections rather than to the public business; and for that reason the editor of the *Electrical Engineer* expressed the opinion that it is hopeless to expect anything in the way of valuable Patent Law amendment from the present Congress. I am inclined to agree with him, and therefore I advocate that this Congress be the Congress that be invited to pass the bill providing for a commission, and that the 51st Congress, which will not be trammeled by such considerations, be the Congress that be invited to enact the bill which shall be recommended by that Commission. It is impracticable, in my judgment, to secure from any one Congress the passage of a bill to appoint a commission, and also the enactment of the legislation which may be recommended by that commission; because the time which passes after a particular Congress assembles and begins business, before that Congress adjourns, is less than fourteen months; and that time is too short in which to secure the passage of our bill or of any bill for the appointment of a commission, and for that commission to pass through its deliberations and produce its report and to have Congress intelligently act upon the bill which may be recommended by that commission. In point of fact, those three things never will be done in any one Congress. Therefore the obvious plan is to ask one Congress to enact a bill providing for a commission, and the succeeding Congress to pass upon the legislation that may be proposed by that commission; and that is the scheme of the bill which appears in your pamphlets as having been advocated by the National Electric Light Association. That bill is proposed to be introduced into the 50th Congress, and the report of the commission is proposed to be submitted to the 51st Congress, so that no time more opportune than the present will ever occur for us to move in this matter. Senator Platt has suggested the inexpediency of a commission, holding it more expedient to apply to Congress to pass particular legislation on the subject. That plan is far more likely to do harm than to do good, because such a particular bill, to amend the Patent Law in a particular way, is the very sort of a bill that will be loaded down with amendments. It will come before Congress with no responsible authorship back of it. It will be an anonymous bill. It will rest under the suspicion of having been gotten up by men with private interests to serve. It will be presented to the committees in Congress, and the committees in Congress will not understand the subject, and they will think they can improve the bill, and they will take a whack at it, and put on a patch here and cut out a piece there; and finally, when that bill is presented to Congress, it will resemble nothing in the heavens above, or in the earth beneath, or in the waters under the earth. And after it has been amended by the individual members, it will bear a still less resemblance to any of the works of nature or of true art; whereas a bill presented by a responsible, authoritative, respectable commission will have weight. It will remedy grievances which are justly complained of by those who are somewhat prejudiced against the Patent system, as well as remedy evils under which those labor who represent the interests of our inventors. Those bills that have been introduced for the purpose of striking a blow at the Patent system during the past five, six or eight years, have all been of one character. I have closely watched them. They are bills which provide that no suit for infringement shall be sustained against anybody for merely using a patented article, which he purchased without actual knowledge that it was covered by a patent. Those terrible grangers from the West, who are represented as being willing to come down on the Patent system the moment they are let loose, like the angel of death, have never made any attempts more extreme than that; and they have made their attempts a little more extreme than they would have made them, if they were instructed on the subject a little better. But they had a real grievance on that point, and members of Congress have thought they were serving their constituents splendidly by introducing bills providing that nobody shall be held liable for the use of a patented article that he purchased for himself without knowledge that it was covered by a patent. Now, those bills have been too broad and sweeping. They could not be scientifically introduced into the Patent Laws without working inharmonious results, and those results were not foreseen by the men who advocated them. Now, I believe firmly, that if an intelligent commission had this matter in hand, one of the things that commission would do, would be to draft a measure upon that topic, going as far as the just requirements of the grangers would carry them, and not going as far as the representatives of the grangers in Congress have gone; and that those sections thus drafted would meet that ground of complaint, and would be seen to meet that ground of complaint by the very men who have made the complaint. Now, I have heard a great deal ever since I attended before Committees of Congress, ten years ago, about a great deal of opposition to the Patent system. This opposition to the Patent system is like the fever and ague you had out West—it was never in the particular town you were in, but you could always find it in the next town. Now, it has happened to me to draw the pleadings, or otherwise participate in patent litigation in every state of the Union, except five, and to argue cases orally in fourteen of the states of the Union, and I have lived among a wide variety of people. When I was a small boy, my father took me from my New England birth-place to Wisconsin, into a purely agricultural community, a newly settled country, where the painted Indians used to whoop by our house in the summer time, and where there were no manufacturing interests, and I say to you gentlemen that I never yet met a solitary man in my life who was opposed to the fundamental principles of the patent system—not one individual. I have met all classes and conditions of men from Massachusetts to California, and I yet have to see the adult voter who is opposed to the underlying principles and general frame work of the Patent laws. We have not had a solitary line of legislation, good, bad or indifferent, on the subject of Patent laws, since 1875; and since 1879 there has been no serious attempt to remedy the evils under which the inventors of America have labored. This is a great wrong, and the United States Senators and the Members of Congress have much to answer for. I see nothing to fear whatever from the appointment of a commission, but everything to hope from it; and I firmly believe that if a commission was appointed, the head of which might be my own predecessor as a writer of Patent Law text-books, Mr. Curtis—if such a commission were appointed to sit as judges, to do justice according to their knowledge of Patent Law, between different classes and conditions of men, between the inventors and the users of inventions, the report of that commission would be seen to be so eminently fair, so eminently sound, that it would go through both Houses of Congress without serious opposition, without any attempts at amendment that would be more harmful than a fly upon the trunk of an elephant, and without any changes whatever. Whereas, if we continue to content ourselves, or endeavor to content ourselves with the outgrown clothes that we have tried to robe ourselves in during the past fifteen or twenty years, we will stumble along after a fashion, and the world will not collide with any other planet, and inventors will get some sort of reward for their inventions; but there will be a great deal of trouble, a great deal of unnecessary loss, and a great deal that ought to be avoided. In these days, when the arts are being so perfected, it is disgraceful to the United States that the Patent laws are not also being perfected. What would anybody think of the suggestion to do away with dynamos by reason of the fact that they are more dangerous than a Morse telegraph sounder. In like manner, how can we estimate our courage at a high rate when we are at all restrained by the opinion of Senator Platt, or anybody else who entertains such an opinion, from doing what in us lies to bring to the attention of the American Congress the evils under which we and those we represent have long labored, and ask that those evils be remedied.

MR. CURTIS:—Mr. Chairman, I may give my friend from Connecticut a single instance, such as he never met with. A great many years ago—before the war—there was a Senator in Congress, from North Carolina, Mr. Badger. He was a very eminent lawyer,—a general lawyer—and a most admirable person. I was conversing with him one day about a bill that was before Congress—I do not recollect whether it was a special bill or what but it had relation to some changes in the Patent Law. Well, he said, all I know about the Patent Law is this. Some infernal Yankee comes down to my country and mouses around my back yard and looks over the fence, and he sees something in use there in my garden, and he calls out to me and says, "Stranger, I have got a patent on that; you will have to pay me ten dollars for the privilege of using it." That, he said, is all I know about the Patent Law, and all I want to know. Well, I said, Senator, you are here to legislate for the people of the United States; you are in the highest branch of the legislative department; now, that is not worthy of you or of the state that you represent. Now, you just take this subject and look into it and you will find that there is a great deal in it that you ought to know and that you ought to be able to give an intelligent vote upon. He did so, and he was entirely convinced that he ought to know something more about the subject than he did. That is the only instance of the kind that I have met with.

MR. PHELPS:—Mr. Chairman, I have a letter here from Mr. Kintner, which I will ask the Secretary to read, and after he has read it, I will have a word or two to say.

The Secretary then read the following letter:

"265 Broadway, Rooms 26 and 27,
NEW YORK, Dec. 17, 1887.

"MR. GEORGE M. PHELPS,
11 Wall Street, City.

"MY DEAR SIR:—

"I am in receipt of a communication from Mr. R. W. Pope, Secretary of the Institute of Electrical Engineers, advising me of the discussion touching the question of a revision of the Patent Laws on the 20th instant, and inviting me to be present and express my views in relation thereto. I had hoped that it would be possible for me to be with you upon this most interesting occasion, but business engagements will keep me in Washington the greater part of next week. This matter is of momentous interest to all inventors and persons interested in patent property. Some years ago, I had the honor to call the attention of the soiety to the wants of the Patent Office and the necessities of material changes in the Patent Law. I have noted with interest all that has been done by the Institute, and am gratified at the fact that the committee appointed by it has succeeded in putting matters in a tangible shape to go before Congress. While the scheme proposed by this committee is in my opinion entirely practicable, I have some hesitancy as to the feasibility of its presentation to Congress at this time, and am in hearty accord with the views expressed by you in your editorial in the December number of the *Electrical Engineer*.

During my official connection with the Patent Office as its Chief Clerk, I very naturally became pretty well versed with the political phases of all matters appertaining to the conduct of the bureau. It is needless for me to say here, that in matters of polities individual personal interests control individual members of Congress, and that upon the eve of a great national election, such as occurs next November, we cannot afford to run the risk of losing everything in a proposed change of existing laws. I, therefore, beg that you will present the following resolution to the society and urge its passage:

Resolved, That it is the sense of the Institute, that it is unwise at this time to seek by legislation to in any way modify the existing Patent system, and that any movement to that end should be postponed until after November 1st, 1888.

I am sure that this resolution will meet with the full approval of every one who has the good of the Patent system at heart.

Very respectfully yours, C. J. KINTNER."

MR. PHELPS:—Mr. Chairman, I present the letter at this time, because Mr. Kintner supplies a resolution and has requested that it be offered, and I offer it and move its adoption, in order to have a more definite thing before us to discuss. I have listened with great interest to Mr. Curtis and to Mr. Walker, but I am still of the opinion that it would be unwise to present the proposed bill to Congress. I think it is quite clear that there is no difference of opinion here as to the merit of the proposed changes—the changes sought to be made in the patent system through the presentation of such a bill. But in respect to the unfavorableness of the time and the public temper—I think there is much difference of opinion on that point. We had better educate public opinion further before attempting to obtain legislation. Mr. Walker has never met anybody who was opposed to patents. I have met a very considerable number of persons who have a distinct opinion unfavorable to the Patent Laws—unfavorable to *any* Patent Laws. I have heard a number of intelligent persons say, that we would be better off if the entire system were swept away; that if the improvement of machinery and methods and processes was left entirely to the competition of manufacturers, there would be sufficient incentive to develop and improve the arts.

Mr. Curtis:—Haven't they got to strike out a part of the Constitution of the United States in order to reach that?

Mr. Phelps:—Isn't the provision that Congress *may*?

Mr. Curtis:—Yes, but it always has been considered an imperative duty.

Mr. Phelps:—Well, however unreasonable such opinions may be, there are a considerable number of people who hold them. I mention that because Mr. Walker seems never to have met an individual of that kind, and I happen to have met a number.

Mr. Curtis:—You may perhaps find a great many people that do not know there is such a provision in the Constitution.

Mr. Phelps:—Precisely. Inasmuch as the editorial in the *Electrical Engineer* has been referred to, I may perhaps repeat here some of the points therein presented. As to this point which is brought out by Mr. Curtis, that many people do not know anything about the constitutional provision for a patent system, there is an immense amount of ignorant prejudice in the public mind. The subject is not well understood; and in view of that and of the constant recurrence of these unsuccessful attempts which Mr. Walker has told us of, to interfere with the Patent Law, the indications are that if an opportunity be offered for renewing attacks all along the line, it would probably be availed of to a very considerable extent; and it is not at all unlikely that a very large number of members of Congress would be very greatly influenced by public opinion hostile to patents. If we could feel quite sure that the commission would be headed by Mr. Curtis, and composed of men like him and Mr. Steuart, we should have no hesitation as to *their* part of the work. The hesitation comes in as to what would be done with it after they had formulated it, and it seems to me we have little to expect from any early Congress. The bills to reform other features of the national polity have not always fared very well at the hands of Congress. I do not know why I should feel certain of having wise and discriminating action taken by that body upon a measure to reform the Patent Law.

Mr. Walker:—The last tariff law came from a commission.

Mr. A. C. Fowler rose to speak.

The President:—I would like to ask Mr. Fowler to preface his remarks with a few of the statistics he has recently compiled with regard to patents covering electrical inventions.

MR. FOWLER:—I have understood from good authority that three-fourths of the manufacturing interests of this country are based on patents, and I think that in the electric art we can safely say it is nine-tenths, because nearly every electrical appliance is patented in one way or an other. Up to date there have been 375,000 patents and nearly 12,000 electrical patents, making one-thirtieth of the entire number of patents granted by the United States electrical. Most of these electrical patents have been granted within the last ten or twelve years. There are about 25,000 applications patented every year—nearly that number—and about 30,000 filed. In the electric art there are about 4,000 applications a year, or about seventy-five per week, averaging about a dozen a day. I think this will show the importance of this subject to electrical men. That is the reason I mention it to-night. It would be safe to say that almost 10,000 of the 12,000 electrical patents that have been granted to date have been granted within the last ten or twelve years, averaging nearly a thousand electrical patents for each year during the last ten years. Before taking my seat I should like to say something upon the paper that has been read this evening. I stand here as a representative—I do not know but what the gentlemen may call it the Patent Office mill—but I have been through it and I have been in the electrical part of it. The gentleman, I think, has dropped into an error about the Patent Office paying all its expenses since it has been organized. In a paper which I had the pleasure of reading before you a year or so ago, I mentioned that an Assistant Commissioner had looked up this point. In years gone by, the printing that was done for the Patent Office was done by an appropriation of Congress and not out of the Patent Office fund. Recently the expenses of all printing have been deducted from the Patent Office fund. This Assistant Commissioner ten years ago, I think, ascertained that, including the cost of printing, the expenses of the Patent Office would have been about equal to the amount of the receipts. From the gentleman's remarks this evening, one would be led to believe that there was no such thing as an examination in the Patent Office for admission. I can tell you all here this evening that there are some examinations held in the Patent Office, and pretty hard ones too. I have seen graduates from the Yale

Scientific School, from the Princeton Scientific School and from the various scientific schools of this country, and from West Point, come down to the Patent Office. They were going to get in, but I have seen a great many of them — the most of them—go back. The examinations they hold there are quite technical. I have been through them, and I have had to study very hard to get through them, and I started with a good scientific education. It has also been the practice of the Patent Office in years past, to make the examination include questions on Patent Law, and previously nearly all the questions were on Patent Law. Latterly they have been more on scientific subjects. For promotion in the Patent Office the questions are largely those of practice and Patent Law. The gentleman will find among the examiners a great many who are quite well versed in the law. There are in Washington three night schools turning out lawyers by the hundreds, and a great many examiners in the Patent Office are students in those law-schools. Now, a gentleman takes exception to one examiner ruling differently from another. You will find the same thing in the courts. The questions that arise in the Patent Office among examiners on applications, are not so much questions of law as they are questions of fact, and where you find questions of fact you will find men differ. It is an old saying, "So many men, so many opinions." It is about true in the Patent Office, and I do not see how it will be possible to harmonize in any way the rulings of examiners. Now I should think the gentleman, from his remarks, expects an *ex parte* proceeding to bind every one. It is well known that an application for a patent is an *ex parte* proceeding. It is not contested at all, and it is no wonder that they are sometimes overturned in the courts, and I think there is always a presumption in favor of a patent. I think the courts recognize a *prima facie* right when there is a patent. This question of Patent Office reform, is a question that has been studied within the Patent Office itself. The examiners there have made a great study of it, and they have thought this question over. Some years ago they took steps in this direction. It was in the particular direction of an increase of salary though, and of course that was considered a salary grab and it was not very successful. The gentleman, I think, has enumerated a large number of objections to the Patent laws that are highly visionary. It strikes me that the system, as a system with some few exceptions, is quite a good one. I heartily concur with him in the wisdom of establishing a Patent Court, and I think he will find the Patent Office itself believes in such a thing. Almost every one believes that the judicial functions of the Commissioner should be taken away and his office made purely executive. It strikes me, as far as the Patent Office is concerned, there are only two things the matter with it. One thing is the inadequate pay of examiners. If the pay of examiners is increased, say doubled—I mean all through the assistants—the tendency will be that men will stay there, and a man that has been any time in the examining corps can perform three or four times the amount of work that one who has been there for a few years can do. It strikes me that this is the one thing needed in the Patent Office, increased pay, and there might be an increase of force. As to the appointment of a commission, it seems to me that the Patent Law could be revised by direct application to Congress. There is a standing committee of the House, and there is a standing committee of the Senate, and the Chairman of the Senate Committee is Ex-Secretary Teller, of the Interior Department, who knows all the Patent Office workings, and who has, in times gone by, had a great many cases appealed to him from the Patent Office, before his jurisdiction was overruled by the Supreme Court of the District of Columbia; and I think that if the National Electric Light Association, or the electrical men, were to appoint a committee to confer with the Patent Committee of the Senate or of the House, that this could be effected more readily than by appointing a commission. It is a more direct application of power than the other way, and I think we all believe in applying power just as direct as possible.

Mr. Phelps:—Mr. Chairman, on one point I wish to add to what I have said. I have had some conferences with other persons in taking a position on this matter. I have not trusted merely in my own views and apprehensions; I have been told by an attorney of prominence and large experience in patents, as well as other branches of the law, that he went to Washington some two or three years ago, with the express purpose of initiating a measure to remove the very objectionable limitation of United States patents by foreign patents, and that having consulted with various persons, in and out of Congress, with respect to the matter, he concluded that he had better say nothing whatsoever about it; better drop it. In his judgment there was a danger then, of inviting attacks on the patent system, through amendments and otherwise, even in respect to such a specific measure as that alone. I confess, that incident has influenced my mind very considerably in forming an opinion upon the expediency of the measure before us.

MR. THOMAS D. LOCKWOOD:—I stand before you this evening the representative of perhaps the best contested patent in the world—the Bell telephone patent of 1876, and yet I like the Patent Law of the United States. I am not in the best condition to talk, because I have not heard the previous discussion, and I have not heard the paper, and I have just come from a dinner of about fourteen courses. Nevertheless, as the last seven years of my life have been spent with greater or less success in studying patents, I thought it right that I should say a word. With all the evils of the Patent Law of the United States, it is yet the best patent law in the world, and my experience is, that the Patent Law is not one-tenth as immoral as patentees themselves are. One man gets a patent for a steam engine. It is the first time, perhaps, that a steam engine has ever been invented. Another man gets a patent for a crank *A*, fastened down to the end of the main shaft *B*, and attached to the reciprocating connecting rod *C*, with a brass bushing in it, *D*; and the second man cannot comprehend for the life of him, why he has not got the right to use the steam engine, and he goes ahead and uses the steam engine, and a suit in equity is brought against him, and when a preliminary injunction is obtained, he poses before the world as a martyr to inefficient Patent laws; and then he goes to work and makes a slight variation in his patent, and gets perhaps a new element in his same old claim, and goes on infringing, and he is prosecuted again, and is enjoined again, and he poses before the world as a double martyr, and then gets a few more of his kind together, and they form an association for the protection of patentees. Then a few gentlemen, who really are patentees of the first order, think it would be a good thing to be protected also, and for the sake of their own virtuous patents, they combine themselves with the immoral patentees, and get a pretty good lawyer, sometimes a first-class lawyer, to frame a bill for the amendment of the Patent Law. In any event a bill is introduced for the protection of the innocent purchaser, and the innocent purchaser, nine-tenths of the time, knows very well that he is purchasing a patented article, because he has got the patent stamp very likely so emblazoned on one side of it that he cannot put it straight on the wall to save his life, but it has got to be hung bias. For these reasons, and many more reasons, I believe that we had better let well enough alone. I have noticed in the last seven years that whenever Congress goes to interfere with the Patent Law, they interfere with it the wrong way. Two-thirds of our Congressmen have got the notion which prevails in Wisconsin and Minnesota and the northern side of Dakota and Idaho, and few more places like that, that the patent system is nothing more or less than a system of wholesale swindling; and I was very glad to see by the Supreme Court decision last May, on the driven well patent, that the Supreme Court of the United States was not going to be influenced by popular notions or popular sentiments. I was glad to see that the Supreme Court could sustain a righteous re-issue. Even now the present Congress, which has not been in existence much more than a week, has begun to get in its work. I noticed last Friday morning that the regular innocent purchaser bill had already started on its march. I am very much of the opinion of Mr. Phelps and of Mr. Kintner, that at the present time it would be unwise to try to get any patent legislation. I am sure that if we stir the matter up we shall stir it more than we want to. If we get a commission, our worthy President, with the best intentions in the world, will nominate two commissioners from Michigan, and two commissioners from Indiana, and one commissioner from Ohio, and I am sure that that commission will consider it its duty to point out what one gentleman very forcibly did this evening, and say that the Constitution does not provide for a Patent Law; it only provides that there *may* be a Patent Law, and they will condemn by ignoring that article of the Constitution which intimates that, if there is a Patent Law, it shall be to promote the arts and sciences. I am very glad that in these days of corruption, of judicial corruption, we have in the Courts of Equity upright judges; judges that are well versed in the law, in Patent Law, and who usually keep that provision of the Constitution before their eyes, and keep the idea of that article of the Constitution before their eyes, that a Patent Law is not to oppress people who use useful instruments, is not to provide that certain advantages and certain monopolies shall be given to certain people, but that in consideration of some useful thing furnished by an American inventor, that man shall be protected in the manufacture of his invention for a certain limited period. Now, with respect to the Patent Office, we cannot deny that there is a good deal of inequality of practice, notwithstanding the eloquent defence made by the gentleman on my left. Those of us, who have practiced before the Patent Office, have sometimes had the ill-fortune to have a case before three successive commissioners, either by an interference or by a double interference, or by some patentee who comes in at the very last minute, or by the inability of some examiner to distinguish between matters of form—matters of law—and the real subject of the case in the class of electricity. Since I have begun to send applications to the Patent Office, we have had the immortal Zenas Fisk Wilbur as first examiner in the electric class. We have had H. C. Townsend. We have had Dr. F. L. Freeman. We have had Mr. C. J. Kintner; and now we have Mr. Brown. Every one of these examiners, to my certain knowledge, has made different rulings upon the same point with the best intentions in the world. Mr. Wilbur used to make rulings, I think, that would help his own pocket out. Mr. Townsend was about as fair a man as I ever knew. Dr. Freeman was first class. Mr. Kintner always recognized the difficulty of formulating a proper electrical claim and allowed considerable latitude. Mr Brown, filled from the sole of his foot to the crown of his head with law, but knowing very little about electricity, is disposed to stickle for matters of form. It may be that the gentleman is here. If so, I assure him that I do not mean any disrespect; but he came to the class of electricity from the class which included chamber-ware. Now, how can a gentleman, no matter how good a lawyer he is, become an accomplished electrician in the twinkling of an eye, simply by being transferred from one part of the Patent Office to another? I do not know. It may be that he passed the competitive examination spoken of. If so, I have nothing more to say. If he passed it, all is well. I hope when Mr. Brown has fully learned electricity, and learned it well, that he will stay there, and that by that time we shall have some provision, either with law or without law, whereby examiners are well paid if they are good ones. I have nothing to say against Mr. Brown's rulings. They have been fair as far as I have seen. He seems to have a disposition to do right in so far as he knows it; but it would be a benefit, if we could get some uniformity and if we could get Congress to allow the Patent Office to spend the money which the Patent Office earns. Once more, I wish to express my opinion that it is not wise in the year before the general election to stir up any general legislation on patents, and I do not think that a commission is the best way to do it.

I think we shall have to go straight to Congress and get some kind of law which is formulated after mature deliberation and consideration by all those having an interest—by a congress of patentees if you will, with the best legal talent we can get.

MR. FRANKLAND JANNUS:—I have listened with a great deal of interest to all that has been said for and against the revision of patent law, and I want to add my mite. I do not think that after all that has been said there is much more to add relating to the Patent Law itself. I agree with the gentlemen who has just sat down, that it is a very excellent affair as it stands; and as the matter appears to me there are only a few things that need immediate attention, but those I think are urgent. I refer first to the entire divorce that I think should exist between American and European applications. We do not pay much attention to European laws. They do not pay any attention at all to ours. And why we should be hampered by them I never could understand at all. Now there is no better illustration of the danger of meddling with the Patent Law than what occurs in connection with foreign patents. Take the matter of the International Convention. That whole thing is absolutely inoperative for the reason that our representative did not know the difference between the filing of an application and the granting of a patent. As I understand it to-day, if you can get a case into the office and if you can get it through inside of six months, then you can take advantage of the balance of the seven months, otherwise the provisions of the convention are of no use to anybody. That is just a trifling example of the intelligence and information displayed by our Legislators on patent matters. No doubt the person who conducted the United States negotiations was perfectly sincere in his intentions. But the result is not good. Then we want something in the matter of re-issues. Mr. Lockwood has referred to *a righteous re-issue*. It is hard to say what that is. It seems to me a re-issue is righteous when the Supreme Court of the United States says so, and not until then. It is a very hard thing to get Congress to decide what a law is after the United States Supreme Court has had a whack at it. Now I think that a great many of the difficulties and troubles from which we actually suffer exist in the Patent Office. I have thought so for a long time and think so still. Reform is needed and we do not want to go to Congress for that, because we will get nothing. If Congress would do anything about the rules of practice, I would like to have them, but I doubt very much that they will. I have never yet known an instance where an Examiner was ousted for incompetence, and there is not a man here to-night who has practiced before the Patent Office who does not know that there are lots who are incompetent. Many are competent, I admit that; but incompetent ones exist. There are men in prominent positions in the Patent Office to-day who got in before the examinations began. They could not pass a scientific examination to get in to save their lives, if they were once out, and the only way a man can do when he knows his application is to be considered by such a person is to get a change of venue in some way or other.

We all appreciate the entire importance of a record of previous cases in considering the validity of a particular patent. We all know that we have to go to the original files; it is there we get the original information. We want to look at the references, etc. Now, it is a fact that when you go back a few years you cannot count upon a complete file in the records of the Patent Office. I do not mean to say there are none, but I mean that back of the few years the files are incomplete. They contain the specification and letters, and probably a printed copy of the specification, but no drawings. Then, as the record room is about a mile away from the file room, there is only one single mode in which you can get the whole case together, and that is, to buy copies of everything you want in order to be able to look at them all together for about ten minutes. I once called Commissioner Montgomery's attention to the matter. He said, "Why, certainly. Mr. Duryea, won't you see to it that these copies are put in these files?" I felt that I had accomplished something. I went up there again in a few weeks and asked somebody if he had heard of this being done. It had not been heard of. I went to see the Commissioner again. He scratched his head and seemed to try to recollect: "Oh, yes; oh, yes," he said, "Mr. James, if you will put that matter in writing, I shall be ever so much obliged to you." I proceeded to do so. In the course of a few days I received a very courteous reply which said, that the matter referred t my communication of such and such a date, had been referred t a committee, consisting of the chief draughtsman, the chief clerk and one of the principal examiners, and that they had decided that, whilst it was very desirable that this thing should be done, still that there was no force at the disposal of the Commissioner with which to accomplish the same; that it was eminently desirable, but that it could not be done, and he kindly concluded with the suggestion that I call the attention of his successor to the same thing. I do not know whether his successor has got around to it or whether he ever will, or whether I ever shall. I think that some of the surplus, of which we have all heard so much, ought to be applied to attending to that matter.

I remember once being particularly struck by the effect of the statement on my part, that the Patent Office was responsible for nothing and responsible to nobody. The more I think of it the more I appreciate that that irresponsibility gives rise to a good many very peculiar actions. It does not make any difference how much an examiner impedes the progress of your case, there is absolutely no responsibility on his part. There is nothing he can do, judging from the history of the office, that will secure his dismissal or reprimand. The only thing you can do is to appeal. That, it seems to me, ought to be remedied in some way or other. A man goes to the Patent Office with a case; he is posted in the art and he knows it is new. His attorney knows it too, of course, as his client has told him. Well, the case is taken up by some junior, some new man, probably in the office only a week or two, which very often happens, especially in the class of electricity. References are not well considered, and the burden is thrown entirely upon the attorney and his client to demonstrate to the office, to explain to the examiner, what the reference does show, and then call attention by contrast, to what the application shows, the precise work for which the Government Examiner was paid. That is a thing of daily occurrence. I think that after a man has shown evident incompetence he ought to be incontinently "fired." I think there are plenty of good men to be had. Now, it is a regular thing for a fellow to examine a case and fire a lot of drawings at you and let you fight it out yourself afterwards. That is not the intention of the rules. That is not what ought to be done. I do not say it is done in every case, but it is done in too many cases, and it is these things that keep the work of the office back and hamper and annoy everybody connected with it. As a result of such actions on the part of the office, I have seen inventors deliberately order their attorneys to take claims out after being advised that they could be eventually secured, by appeal if necessary.

The peculiar views taken by inventors of *their* patents, and some other fellow's patent, is very well illustrated by the gentleman who preceded me. And that is true. But that ought not to be the case with examiners, although it is. They take just precisely the opposite view. They think they are counsel for the public, and instead of seeing what is in an application, and really making an effort to cite references to show anticipatory matter, they simply think they can expand a reference to any extent and that is the end of it. If it is rejected they are glad. That ought not to be.

Then this shifting of examiners from division to division is another source of delay and trouble, as in the class of electricity, where the chief examiner comes from water closets to electricity. It practically blocks the progress of that class for a long time. Then, when you come to add his lack of familiarity with the subject, an overwhelming disposition for technicalities, why altogether, you get a very interesting combination.

Now, in the opposite branch of the office — chemistry — there is an examiner, a very nice fellow outside of the office, but then he is always ready to deny the operativeness of a process. He is not going to find out whether it is operative or not. The fact that he will in all probability be proved wrong, does not deter him. You have not proved it is operative. You have not submitted specimens, and that is the end of you until you do. I have cases there now and I know that they are operative. The man who says they are, is a man of fifty years experience; yet this man, with no experience at all outside his room, says it is not operative, and the case is clogged until it becomes convenient for my client to prove otherwise. I have heard it suggested by a friend who has been very highly spoken of here to-night, that the day of a close examination in the Patent Office was gone, that the system of examination — the theoretical system at present in existence — is obsolete. His idea is that the examiner should take up a case, point out objections — insufficiency of description, and all those things just as he does now, and then cite all the references he has, tending to show lack of invention; that after receiving those references it should be the duty of the applicant to amend his case as he might see fit; take out his claim, limit it, expand it, do anything he chooses, and then send the case to issue. That struck me at first as a most iniquitous proposition; but I think it will bear consideration, because the Courts will be the ultimate tribunals before which that case will be settled. For when the office has cited its references, then that branch of the case should be closed and sent forward. The applicant takes the risk in it. The favorable dictum of the Patent Office does not hold before the Court. I think that there is where the responsibility falls in view of the fact that the Courts constitute the only ultimate tribunal. It is not customary for people now-a-days to put money in patents without at least looking at the file, and the moment that is done, they are in possession of the data upon which the Court would have to decide and upon which they might as well decide also.

Now, I think there is another point that we need to consider more and that is this question of trials before a jury. I think we are agreed that the average jury is not fit to pass upon a patent. It takes a great deal of study for men who make it a speciality. I think it would be better to impanel a jury of experts, and pay them as such and let them advise the Court. The trouble with expert testimony at the present day is, that you have the jury and also have the experts, and the experts for each side color their testimony just as much as they possibly can. Then the tribunal not only has to consider the facts in the case, but they have got to estimate just how much an expert can exaggerate, without doing anything more.

I certainly agree with Mr. Steuart that the Patent Law needs some revision, but I am a little doubtful whether this is the best time to do it. Some think it is, some think it is not. I think, if we were to go for two or three inoffensive amendments, we might get them. If that cannot be done, as many gentlemen seem to think, then I favor the plan of a commission, because going to Congress with individual bills always gives rise to the suspicion that the parties who go have an axe to grind. The question is, How can these lawyers afford to come here and ask for these things? People in Congress ask these questions as well as other people. The answer naturally is they are trying for legislation for some particular thing, and especially are politicians liable to those suspicions. They think of that first, last and all the time, judging from what they say, and for that reason I think the plan of a commission would be the best, because the matter can then be presented to Congress in an impartial manner. Whether this present time is or is not the best, I do not know, but I think if it is decided to apply for a commission, that this time is as good as any other. It will certainly take time to consider the question, and to formulate what they want. I doubt very much whether the matter could be brought to the attention of Congress until after this bugaboo of the presidential election has passed.

MR. LOCKWOOD:—I crave your indulgence for one or two more remarks. Words would fail me if I should attempt to depict the interest with which I have listened to the remarks of the gentleman who spoke last; and while I cannot agree with every one of his propositions, I may say that I can cordially agree with some of them. For example, I can agree most cordially with his proposition that the aspect of our Patent Law, with relation to foreign patents, does need revision. But I do not think that a commission is the best means of accomplishing it.

With respect to the re-issue question, I think that the gentleman, apparently, so well able to explain his ideas of what is right and what is wrong, could scarcely find any trouble in ascertaining for himself what is a proper re-issue and what is not a proper re-issue. If we stick to the definition of the statute as to what kind of a patent may be re-issued—that is a proper re-issue. If we take a re-issue patent that is re-issued, obviously after the state of the art is advanced up to a certain pitch, far beyond its reach at the time when the patent was originally issued, and in which the first patent is clearly so reconstructed as to show an attempt to cover subsequent improvements, that is an improper re-issue. I do not think the law needs any revision with respect to re-issues.

The gentleman said that he never heard of any incompetent examiner being dismissed. I never did either. I do not think anybody ever did. It is the competent examiners that get out of the Patent Office. It is the incompetent ones that stay in. But I would like to add a doxology to his remarks there. I think it is equally necessary that there should be some kind of an examining committee for attorneys. I never heard of an incompetent attorney being dismissed permanently. I have known a good many to be dismissed from one client but simply to catch on to another one. And it is a pretty safe rule I think to fight pretty shy of those gentlemen who flood you with circulars after you have got a patent. It strikes me that there is something in the matter of Patent Office tribunals that needs rectifying. I do not know how the law is going to get around it, because the patentee seems to think his rights are infringed, if he is not allowed to handle the tribunals of the Patent Office as he sees fit. If we get an adverse decision, we feel very badly if we cannot appeal to the board of examiners in chief, and so on. But I do not know with my present information where to take hold of this subject of a tribunal. I think that needs a little more attention perhaps than has been given to it. Now all of us who practice before the Patent Office or are interested in patents should be prepared to bear patiently with the custom of examiners who deal with applications in an impartial and square manner. If they allow a patent which afterwards turns out to have other references, and which is not new, I think we should remember that it is utterly impossible in the present state of knowledge, and in the large expanse of publications and of prior patents which they have to wade through, to cover the ground every time. We have got to take our chances in the matter; and those of us who have taken out patents for inventions know we have got to get out patents for the inventor by hook or crook. As a rule the inventor does not care whether the claim is valid or can be sustained or not. If he has got a patent he can easily sell it. My experience is, no matter how foolish a patent is, how unduly broad a patent is, how ridiculous a patent is, all you have got to do is to find somebody with more money than brains and you can sell that patent, and you can organize a company with seven millions of dollars of capital on it, just as easily as you can with the best invention ever made. So I think we must not be too hard on the examiners. There are a good many ways we can help them. As a rule an examiner is open to conviction; the average examiner is, I am sure. About five years ago I filed an application for a patent before a certain examiner, which I think I could have made to cover broadly the conversion of electricity of high potential to electricity of low potential, for the purpose of getting incandescent lamps from the high potential circuits by means of induction coils; and the examiner, he was honest, but he was not informed on the subject, and he said—" It cannot be done." He said there was no such thing ever heard of as working an induction coil backwards well; it was within the extent of my electrical knowledge that it could be done—that it had been done, for I had done it myself, and moreover, I knew that the conservation of energy would work almost any way backwards or forwards. But I had a gentleman for a client who was afraid to give me rope, and the application was put upon a shelf. In such cases as that the examiner is not to blame for sticking to his views; but the client is to blame for not sticking to his views. For my part rather than erase a claim out, to which I knew I was entitled, I would let it stay in the Patent Office until the crack of doom.

MR. FOWLER:—I think, if the National Electric Light Association were to present some bill in Congress, which would increase the pay of examiners say fifty or one hundred per cent., it would go farther towards removing the evils of the Patent Office than everything else combined. In one year we counted that from among the examiners twenty-three had resigned—about one sixth of the whole corps. The Patent Office now is filled with new men. Until a man has examined for one year, he is absolutely worthless in the Patent Office. I have been troubled a great deal myself since I have been out of the office with these questions of form, and I know exactly what it means. If gentlemen here had any conception of the work that is thrown upon examiners, they would know what it means. It is a great deal easier to raise a question of form, than it is to go down and examine the merits of a case. These questions of form are raised by men who are so banked up with work that it is anything to get rid of a case. If the salaries of these men are raised, and men kept there who can perform three or four times the work that new or inexperienced men can perform, reform will follow. I was talking with an examiner, a friend of mine, whom I met in New York a few days ago, and he said this is the way he works—and I thought it was a very poor way—it is anything to shelve a case. He has little rollers—stamps—that he rolls across a sheet of paper—division, process and product and everything else—*ex parte* something—he rolls them over the sheets and hands them over to the clerk, and she writes a letter. That case is got rid of. I was very much struck with this question of form when I was in the Patent Office. There was a case which had been on the merits twice to the board and once to the commissioner, and it was a case we were very much harassed about. It was a question whether the thing was patentable, and one of the gentlemen in charge asked me to take hold of the case and make an examination upon the merits. I took hold of it, spent some little time at it and I found two English patents for exactly the same thing, and yet the case had been appealed to the Board twice and to the Commissioner once upon the question whether the thing was patentable. That just illustrates the point exactly. They will raise these questions of form to get rid of the case rather than examine the merits, because it takes too long. They want to be up to date with the work as near as possible.

Now, this question of limiting American patents by foreign patents does not strike me as being such a terrible thing. It is easily gotten around. It is the popular impression that all foreign patents limit an American patent, and yet it is only so when a foreign patent is previously patented. It is easy enough to file your foreign patent on the same day that the American patent issues, and the foreign patent does not limit the American patent, nor does the American patent limit the foreign patent in any way.

I am a believer in compulsory licenses as they have them in England. I know my friend on the right will not agree with me in this. I do not think one man should be able to get a broad claim allowed and prevent improvers from using improvements. That is what the law of this country gives them. In England a Board of Commissioners ascertain how much a license is worth; and they make a man with a foundation patent give a license to another party, so that an improver can use his invention. There are many companies in this country who have valuable improvements locked up in their safes. In that way they impede progress. I have in mind one company where the managers do not want to change the machinery because of the cost, although there are a great many inventions in their line that are better than the devices they manufacture. They buy up the patents and put them in their safe.

MR. JANNUS:—I did not have the slightest intention of reflecting upon the entire staff of the Patent Office, because I know there are plenty of good examiners; I merely referred to those that were not, and to the fact that, despite the examination for admission, the force is not as good as it might be. I fully agree with what has been said about raising salaries, and I fully endorse the proposition that that would, in a large measure, modify present troubles in that respect. They want more men, and they want to have them more competent. Both things are necessary.

Concerning the new tribunal that has been proposed, I think everybody has considered the question more or less, and I believe everybody considers it more or less of a good plan. I agree also with Mr. Lockwood that this proposition needs to be considered very carefully. As the matter now stands, a person who has lost his case before one tribunal would feel personally aggrieved if he had not the opportunity of carrying it to another.

In days gone by, the assistant commissioner was taken from the ranks of the office. To him matters of form were referred. That precedent has been overlooked and that office has been practically vacant—that is what it amounts to. The question of re-issues is a very serious one. I passed it over, as I did other matters, believing it to be almost too late to go into it; but there is one point in that question I would like to bring up, that is, the trouble in the way of an inventor, in securing what has been apparently lost by a defective patent, principally owing to the fact that it is a conclusive presumption that everything that is shown in a patent and not claimed is patented. If there is any way of overcoming that, then we will have actually what re-issues were supposed to cover. I do not believe that the re-issue law was ever meant to cover a case where a man went in and expanded an application after he had found the desirability of doing so. I do not think it was ever intended, and the Supreme Court evidently does not; but I think that if re-issue is confined to the subject matter found in the claims actually patented, giving it perhaps a better expression or some greater scope, a scope evidently due to it, and that then the applicant can remove from the defective patent the subject matter of a different character which was not covered in those claims, then relief can be found. As the matter stands it is a fact, and I think it will not be denied, that it is almost impossible to do anything with an invention after it has been badly patented. But as I said before, you never know what is a good re-issue until the Supreme Court has passed upon it. That question I feel to be too deep and too serious for our discussion to-night and therefore will leave it.

MR. J. A. MILLER:—I should like to say a few words. I have practiced before the Patent Office since 1854, and I do not know that any attorney practicing before the office has been more unfortunate, particularly in one room, in his intercourse with examiners than I have been. I speak of the textile room. Now, I have felt all these difficulties which have been spoken of by the various gentlemen here and have suffered considerably. At times I was almost disgusted with the actions of some of the examiners, but about four or five years ago I had three very important cases which were rejected in Germany and I went to Germany to appeal. Since that I have been satisfied with our Patent Office and with the practice in our office.

MR. JANNUS:—An ex-commissioner had the same experience recently.

MR. MILLER:—As to raising the salaries of examiners as a whole, I never did believe in that. I believe more in the European system of putting a high premium on the higher grades. I believe in paying the chief examiners a very high salary, but I think the young men can get along with a lower salary, and I think the present salary is sufficient for them until they have striven and gained a higher position. There is another remark I wish to make on this question of a Court.

We have all suffered very much by appealing from the Board of Examiners-in-Chief to the Commissioner and not getting quite to the Commissioner. In fact we do not know where we are when we get there. The examiners-in-chief are very honest men, but they get into ruts. If you have an argument before them, you know just what you are to say and it depends on who is to write the opinion, and if you happen for instance to take the question of employer and employee, and get started wrong on that, you cannot win to save your life.

MR. DAVIDSON:—I submit there is no system for the administration of affairs among men that will not be open to numerous objections from—may I say—theoretical perfectionists. Now, the Patent system is not perfect; we will admit that. There are perhaps many ways in which it may be improved. There has been quite an extended discussion as to the imperfections of examiners. We attorneys are perhaps quite as much as the examiners tainted with original sin. We are not perfect. Our judicial system is not perfect. The question we must come back to is this—is not our Patent system as we have it to-day the best that we can get, with some trifling corrections, and would it be wise for us to open the door to a general revision of the entire system? Is it well to invite changes which will require interpretation, which will require decisions to mark down the lines upon which they are to be interpreted, and overthrow doctrines in regard to patents based upon some three thousand or more judicial decisions? I think not. As soon as a revision of the laws is invited by a commission such as has been suggested, every man with a hobby in regard to the Patent Law, will appear before the commission and seek to grind his own axe. We will have before such a commission all the gentlemen who are in favor of protecting innocent purchasers. It seems to me I may be permitted to bring the discussion to what I understand to be the real point of it, that is, the propriety of making an application to Congress for the revision of the laws. Everybody assumes there are some imperfections; I should think we ought to treat them like any other business enterprise. No business man goes to another and says, I have got a claim against you; I don't know just what it is, but if you will help me to find out I think we can get at it. That is no way to get at it. We ought to decide what we concede to be the points to be secured for the improvement of the patent system, and then when we know just what we want, ask for it; and let the manufacturing interests, and the inventors bring their influence to bear upon the politicians. I believe in practical statesmanship. I do not believe in ideal statesmanship. Let us select those things which are obtainable and make a direct application for them.

Mr. Wheeler:—I think that some time ago Mr. Phelps introduced a resolution, and in view of what has been said, I would like to second it.

The President:—While the Secretary is finding that resolution, I think it would be simply due to Mr. Steuart to afford him the opportunity of replying to the remarks that have been made.

Mr. Steuart:—Gentlemen, I have very little to say in this matter. I came here for the purpose of hearing what you had to say, and I prepared my paper for the purpose of making a text for this discussion, and I am very glad you have so freely and so fully expressed your opinions. There seems to be a somewhat equal division of opinion with reference to the proper course of procedure. I cannot help adhering to my original view with reference to the best course. It seems to me that if we can introduce into Congress a bill providing for certain specific changes in the law which is the result of the work of a competent commission, who have investigated the subject carefully, and who bring in a report which comes from neutral men, unbiased by any personal interest and unbiased by any special class-interest, in such a form that it cannot be considered as class legislation; that we will stand before Congress with a better chance of accomplishing the results that we have in view than we could possibly have were we to go now before Congress, or any other time, asking for specific changes in the law proposed by ourselves. I think much good could be obtained by a bill that would provide for any one of the suggested changes which have been discussed. If a bill were passed providing for an increase of salaries to the examiners, it would do great good. If a bill were passed for creating a tribunal it would do great good. But when we are going about the thing at all, it seems to me that we want to put ourselves on the floor of Congress in the strongest position that we can take there, and the way to do that, as well as I can understand it, is to go there with the report of a neutral commission, and we will have a better chance than we could possibly have with a bill prepared by any class of men, whether lawyers or manufacturers. I will say in conclusion that I think the resolution that has been introduced is unnecessary, because I do not know that it will have any special object or any special effect—any special purpose. The meeting of to-night was called for the purpose of having the discussion which we have here engaged in, and of seeing what was the best that could be done. It is preliminary in its character. There will be a convention of lawyers and manufacturers held in Washington the first week of January, which will be under the auspices of the Patent Bar Association of the City of Washington, and I will be very glad, and I am sure they will be very glad, if all of you who are interested in the subject will attend that Convention for the purpose of hearing the question discussed as elaborately as it will be there discussed. The meeting will last several days, and there will be representatives from several cities. I think, that in view of that meeting and of the very much more full and elaborate discussion that will there take place, any action upon this resolution at this time would be premature.

THE PRESIDENT:—I think it lies within my duty to say that the resolution says, "it is the sense of this society." This is a special meeting, and I do not think we could pledge the Institute one way or the other, and I would like to point out in addition to that, that we have on the Secretary's desk a bulk of correspondence from some of the leading inventors and patentees and patent experts of the country, embracing various opinions; and certainly, before we express a definite opinion one way or the other of the Institute, it would be well to know what lies in that correspondence which should guide our action in addition to what we have heard this evening. I merely throw that out by way of suggestion.

Upon motion of Mr. Fowler the resolution was laid upon the table.

The following communications were received from various gentlemen who were unable to be present at the meeting:

NEW YORK, December 16th, 1887.

DEAR SIR:—Your communication of December 13th, in regard to a proposed revision of the Patent Law, is received.

It will, I regret, be impossible for me to attend on December 20th, a previous engagement preventing.

It is my opinion, however, that it would be exceedingly disadvantageous to attempt to meddle with the Patent Law at present. You are more likely to make it worse than better, by any proposed legislation. Nor do I think the plan of a commission would be a very feasible one. I doubt whether a commission, favorable to patents, could be appointed; and I doubt whether, if appointed, its recommendations would be adopted by Congress.

There is, however, a possible and simple improvement which would be greatly to the advantage of the patent system, and which would not involve the necessity of going to Congress. The present system of taking depositions in patent cases amounts, very frequently, to a substantial oppression of the weaker by the stronger, and, invariably, to a very great and unnecessary expense. The province of the court is constantly usurped by experts—witnesses are constantly asked in regard to matters which, legally, are totally immaterial.

In my experience, over one-half of the testimony in a patent case, on an average, is immaterial, and would be ruled out at a trial at Law. The reason of this is, that the testimony is taken before an irresponsible officer, without power to rule upon questions or answers. Very frequently the Examiner in Chancery is subsequently appointed Master on the accounting, when he immediately has the very power of ruling on the questions which upon the last hearing he did not have. The testimony so taken before him, as Master, is competent to reverse the previous decision on the coming in of his report. I believe, of course, that the best system would be a trial of all patent cases in open court. This, however, is impracticable, owing to the lack of judicial force.

I believe the next best plan, and one that is perfectly feasible, is that all depositions in patent cases should be taken before a Master or Referee with power to rule upon the admission of evidence; and that his ruling should be final, unless an appeal should be taken therefrom, within a specified time, to the court; and upon such appeal I should give to the prevailing party substantial costs, so as to prevent the overburdening of the judges with useless and vexatious appeals. An appeal from the judge's ruling to lie, of course, as it does now, to the Supreme Court of the United States.

I have no doubt that the introduction of this system would decrease the cost of patent litigation, which is now burdensome, over one-half, and could but result in benefit both to litigants and to the courts. The records being much less voluminous would involve much less time for consideration in the courts, and consequently business could be more rapidly disposed of. This modification of the practice would, I know, from discussion, receive the approbation of many prominent members of the bar; and it is a change which could be made under a Supreme Court rule without the necessity of going to Congress at all.

Of course, I appreciate as much as any one, the numerous defects of the present system; and I am sure that that system ought to be changed in many important particulars; but I likewise believe that it cannot be done at the present time, and that any interference by Congress with the Patent Law is more than likely to result in disaster.

Yours truly,
E. N. Dickerson, Jr.

Washington, D. C., Dec. 16, 1887.

The subject of Mr. Steuart's paper on Revision of the Patent Law is an important one, and I am glad you have extended to the absent members of the Institute an invitation to state their views.

Allow me briefly to state that I have no fault to find with our Patent Laws, but that I should like to see:

First: That the Commissioner and the Examiners should receive double their present salaries, so as to induce them to stay in service longer than they do now.

Second: That a Patent Court be established, constituted somewhat on the plan of the Supreme Court of the United States, who shall decide all patent questions, and whose decision shall be final.

THIRD: That no citizen of a foreign country shall obtain a United States patent for a longer period than a citizen of the United States can obtain in said foreign country.

Yours very truly,

E. BERLINER.

LEWISTON, MAINE, Dec. 16, 1887.

I hope that in the discussion of Mr. Steuart's paper some one will again call attention to the point raised in an editorial in the *Electrical Engineer* for December. The probability is, that what we shall get will not be what we ask. And there are one or two features about the proposed changes which I hope Mr. Steuart will explain in greater detail.

For example; the testing laboratory, first suggested, I believe, by Mr. Kintner, and generally received with favor. I am unable to see how much could be done with it without giving to the examiners the privilege of requiring, whenever they saw fit, that the actual device should be brought before them, in complete working order. Would not this in many cases work great hardship?

I understand the Electric Light Committee to recommend a special training for Patent Office employees, similar to that which army officers receive at West Point; but I do not remember that they suggest anything corresponding to the post-graduate course in Indian fighting taken by the West Point boys. It may be, that I radically misunderstand the committee, but it has seemed to me, that, when the skeleton they bring forward shall have been clothed with flesh by professional law-makers, it will take the form of a goverment school of Patent Law and Applied Science, whose graduates would receive a sort of sub-lieutenant's commission in the Patent Office.

Of course, any plan resembling this would in time fill the building with men specially trained for their work. But would they not be likely to be also thoroughly out of touch and out of sympathy with the practical life of the outside world, likely also to have a feeling of contemptuous dislike for the improperly educated majority of the inventors with whom they would be brought into relations, likely in short, to be prigs, pedants, obstructionists, adepts in the art of how not to do it?

Respectfully yours,

ALMON ROBINSON.

LYNN, Mass., Dec. 17th, 1887.

Though I regret the inability to attend the special meeting, December 20th, and take part therein, I am in hearty sympathy with any movement calculated to improve our Patent system. That it has some serious defects, is less of a suspicion than a fact. Just what these are, and how they are to be remedied, is far more difficult to determine. Some of the inequities of the system are, however, discovered without much difficulty and have often been pointed out.

Others, more difficult to locate and more difficult to appoint a remedy for, are none the less important.

The whole subject of the relations of home patents to foreign patents, of the rights of foreign and American inventors comparatively, one to the other, needs remodeling, in my opinion.

I believe that the standard of what constitutes an invention must, in the natural growth and development of the arts, gradually undergo a change, an elevation in such standard. It cannot be denied that to-day, with the experience and resources which have accumulated, many things are produced as the simple result of mechanical skill or skill not involving invention, and that very many such things become the subjects of patents, which, instead of benefiting the public by disclosure of something new and original, serve only to check legitimate growth of industry and harass the true inventors. How often, indeed, do we find inventors themselves applying for and taking out just such patents as involve no invention, but only aggregation and mechanical skill, knowing full well that they must do this in self-protection, to prevent being harassed in the development of their real inventions, by the *pseudo* inventor or *quasi* mechanical improver.

Many are the instances in which this is done, and for protection the inventor must not only take out a patent for his invention, but must give the time and energy and money to taking out his *pre*ventions of *circum*ventions. For instance, one inventor takes out a patent for, say, a dynamo, belted or connected with a motor, in a certain combination, and shows the two belted together. Another inventor comes along and patents in the same combination a dynamo and a motor whose armatures are on the *same shaft*. Numerous such instances might be given. In fact, I have a case in mind in which the prior inventor shows both belting together two machines, and placing them on the same shaft, and using them in a certain combination; while another subsequently gets a claim limited to the same combination, but with the machines *on the same shaft*. It is not sufficient to say that such a claim is invalid, because it involves no invention; for the patent, having been issued after an *examination* of its novelty, has a certain warrant upon its face as containing invention, and with the sanction of the Patent Office.

As a consequence of this condition of affairs, it often happens that the intelligent inventor, or his attorney, who can and does discriminate as to what is invention and what is not, fails to claim what the less discriminating inventor or less careful attorney easily obtains. The result of this condition is to make the "claim everything," course the only safe one, assuming the Patent Office to be uniformly consistent in its rejections, an assumption which is theoretically, though probably not practically true, bearing in mind periods of time which have given rise to changes in the officers in charge.

There is much more which might be said, but I will content myself for the present with the above references to what I consider a defect arising out of our patent system, leaving to others to point out such other imperfections as may have come to them in their experience, and of which I shall be an interested student.

I seriously doubt whether any patent system can be devised which will not, in many particulars, become outgrown in a moderate period of years. Our present system seems, in many respects, to have been so outgrown, and if not revised, will become more outgrown as the needs of the industrial arts develop in new lines. What was formerly a real invention becomes, with the greater spread of knowledge and skill, merely the exercise of that knowledge and skill to fill a need for a new device, a need which in itself may not have existed prior to the time of its being filled. Indeed, this stage of progress seems to me to have been reached in certain fields of work.

Respectfully yours,

ELIHU THOMSON.

BALTIMORE, December 19th, 1887.

I regret that press of other business prevents my taking part in the discussion on a subject so important to the interests of inventors, and to the industrial and commercial welfare of the country in general, as is the movement about to be undertaken for a revision of the Patent Law.

That this law is sadly in need of revision, is patent to all who have had any experience in connection with it, and its principal defects are sufficiently well known to render it unnecessary to refer to them here, since this feature of the subject will doubtless receive its full share of attention at the hands of the able gentlemen who are assembled to do it justice.

There is one point in connection with this subject, however, that I think calls for concerted action on the part of the members of the Institute and inventors in general, inasmuch as it has an important bearing upon the question. I refer to the recently ratified International Convention for the Protection of Industrial Property, and its relation to the proposed revision of the Patent Law of the United States.

The inducements offered to American inventors by the Patent Laws of several of the more progressive foreign countries are now comparatively liberal, and it is but natural that, in view of the growing popular demand for inventions of merit both at home and abroad, the number of foreign patents taken out by citizens of the United States has largely increased. The remarkable and successful progress that has recently been made in the various applications of electricity, both in this country and in Europe, has opened up a new and profitable field for further effort in this direction, which electrical inventors in this country have not been slow to occupy. In fact, so great is the spirit of rivalry and competition in electrical matters at the present time, that the legal protection of an invention in several different countries is almost a matter of necessity in order to render it commercially valuable.

It has been customary for inventors in this country to file applications for foreign patents on the same day upon which the United States patent issued, or within a few days thereafter. This course was necessary, first, because of a provision in the United States Law (Section 4,887, Revised Statutes,) limiting the duration of the patent, if the invention is first patented in a foreign country. (As the date of filing is also the date of patent in the principal foreign countries, this fact has been construed as being sufficient to cause a limitation of the United States patent.) Second, because in several of the foreign countries a valid patent cannot be obtained, if a description of the invention becomes accessible to the public before the specification is officially filed. The compulsory filing of the foreign cases before the issue of the United States patent, under the provisions just cited, is a serious hardship to the inventor, because it necessitates the expenditure of a large sum for patents before he has had an opportunity to test the public feeling in regard to the usefulness or value of his invention, or the probable demand for it in those countries in which he purposes applying for letters patent. Furthermore, if an inventor intends to apply for foreign patents upon an invention pending in the United States Patent Office, he is compelled to abstain from doing so until after the various and sometimes tedious delays that frequently occur before the case is finally allowed and passed to issue. Moreover, an inventor is prohibited during the pendency of his United States application from publishing an account of his invention or discovery in order to publicly establish his claim thereto, or for any other purpose, because of the risk of losing or endangering the validity of the foreign patents when they are applied for.

Among the provisions of the International Convention for the Protection of Industrial Property, the outcome of a conference held in Paris by the delegates of several European countries for the purpose of introducing certain reforms into the various patent systems with a view to promoting harmony between the same, is a clause allowing an applicant in any one country a period of six months (an additional month being added for countries across the sea,) from the date of filing his application, within which to file the same in any other country belonging to the Union, and that during such period his rights shall not be invalidated by any publication or public use of the invention. Unfortunately, while the intention of the conference was good, the clause under discussion does not conform to the United States patent system, and is directly in opposition to the statutes as they now exist and in accordance with which patents in this country are issued. Indeed, it is a question whether, if the recent adoption of the articles of confederation by the United States are binding, the rights heretofore enjoyed by American inventors are not further restricted, since unless foreign application is made within seven months from the *date of filing* in this country, the foreign patent may be refused or invalidated. If this be the case, it is needless to say that *fully nine-tenths* of all the electrical cases now on file in the United States Patent Office are debarred from foreign protection in any of the countries of the Union, and the electrical division is now about six months behind on new work.

It is clear that such a state of affairs cannot long be allowed to continue, and it is high time that the entire patent system, not only of this, but also those of other countries, be placed on a firm and reliable footing. This can best be done by taking steps towards properly amending the provisions of the International Convention, so as to adapt the same to the requirements of our patent system, and these steps should be taken either separately or in conjunction with the proposed revision of the United States Patent Law. The international phase of the question will probably permit of a more ready solution by avoiding the attacks that may be made upon our present patent system, if the matter is taken up by Congress in its local bearing only, while by having proper representation at the next International Conference, sufficient progress may be made to introduce a number of important changes for the better without the necessity of re-opening the entire subject.

With regard to the changes to be made, I would respectfully suggest, first, that the latter part of section 4,887, Revised Statutes, before referred to, be stricken out, as it is of no particular advantage and is detrimental to the interests of American as well as foreign inventors. Second, that that action of the International Convention relating to the grant of a period of six months from date of filing in one country within which applications must be filed in other countries, be amended so as to allow applicants *twelve months from the date of publication of the invention* in any country of the Union, during which the invention will be protected as at present provided for. This will give an inventor one year after *publication* of his invention, whether as a patent or in a newspaper, within which to file his foreign applications, and would also enable him, if he so desired, to make foreign applications during the pendency of the United States application, which is sometimes of great importance, especially in case of any delay in the United States office, as in the event of interference proceedings and the like. Another point that might be touched upon in an international treaty is the abolishment of annuities, and if possible, the further reduction of foreign patent fees. Also the erasure of all laws relating to the "working" of patented inventions within a specified time. It is to the self-interest of the owners of a patent to work the same as early as possible if it is worth working at all.

Very respectfully,

ELIAS E. RIES.

NEW YORK, Dec. 20th, 1887.

I regret very much that I shall be unable to be present at the reading of Mr. Steuart's paper on the Revision of the Patent Law, and to listen to the discussion that will follow. The subject is one that cannot fail to command attention from all who are interested in any way in inventions, and especially electrical inventions. The growth of the electrical arts is so recent that its history and its causes can be traced with comparative certainty; and no one who has made the slightest inquiry in this direction, can have missed the conclusion that our patent system has been the most important factor in the development of the arts of electricity. To such an extent is this the case that a true friend of the cause might well deprecate any changes of the law, certainly any sweeping ones, as being fraught with more danger than the present condition of things. There are, however, certain matters of Patent Office administration that ought to be corrected, and there are certain tendencies of legal interpretation, adverse to patents, which can only be set right by amendments of the statutes. The needs of the Patent Office in the way of room and examining and clerical force, have been long appreciated by inventors, and, for that matter, have been forced upon the attention of Congress by every Commissioner for the last twenty years.

How to make the practice of the office uniform, is another question of great importance. One Commissioner, at least, tried to accomplish this by forming a court of consultation within the office itself. Discussions were had between the different examiners in the presence of the Commissioner, and he gave the full influence of his authority to bringing them to harmony, but I understand that very little came of it. I believe that something in the nature of the Patent Court, proposed by the National Electric Light Association, is the best thing yet brought forward for securing the result.

These and other matters of importance, will no doubt be fully discussed at the meeting to-night.

Another thing, however, which will be called to your attention, seems to me of supreme importance. It relates to the trend of recent Court decisions in re-issue cases. I do not refer to the two years' limitation, but to the dictum that an inventor is presumed to dedicate to the public all that the incapacity of his attorney or the stubbornness of the examiner causes to be left out of the claims. Down to the present time, this has been asserted only in re-issue cases. Carried, however, to its logical issue, it would apply also to original patents and would call for a strict interpretation of every patent claim, by itself considered, and without regard to the specification. This, if not already the confirmed rule of interpretation, will be the inevitable result of the present tendencies. The claim must stand by itself, and, whatever the specification may say, it will not broaden a claim; everything not clearly claimed, will be regarded as abandoned to to the public. Two citations will show what I mean. "But it must be remembered that the claim of a specific device or combination, and an omission to claim other devices or combinations, apparent on the face of the patent, are, in law, a dedication to the public of that which is not claimed. It is a declaration that that which is not claimed is either not the patentee's invention, or, if his, he dedicates it to the public." Miller *vs.* Bridgeport Brass Company.

And: "It is doubtful whether a re-issue patent can be sustained in any cases where it contains claims that have once been formally disclaimed by the patentee or rejected with his acquiescence, and he has consented to such rejection in order to obtain his patent." Leggett *vs.* Avery.

Now, what I wish to say in view of all this is, that it is unjust in the highest degree to hold an inventor responsible for the errors of judgment or the obstinacy of Patent Office examiners. I do not believe that any just complaint can be made, if inventors are held strictly accountable for the mistakes or incapacity of their solicitors; this is a matter they must see to for themselves. But it will go hard if the Government does not also make itself responsible for its own representatives. When a man puts in an application with narrow claims and makes no attempt to get more, let him suffer for it, unless he takes speedy measures to remedy the neglect. But when the record shows that an inventor has asserted his claims with enough persistence to prove himself in earnest, it certainly should not be charged to his account, if he is compelled to accept less than he deserves. Nor should it be necessary for every inventor to carry his case by successive appeals to the Court of last resort; it should be sufficient for it to appear that he yielded to a confirmed practice of the office.

There should be some law framed to make the Government entirely responsible for any abuse of power or any false judgments relating to patent applications on the part of examiners. I would have this extend also to cases of fraud. It is not good morals and ought not to be good law for one party to a fraud to repudiate the consequences of it. Whenever, by reason of fraud, a patent issues to one who is not the first inventor, the wrong can be righted by legal means already provided. But whether there be fraud or not, the Government cannot justly disclaim for itself a responsibility which it insists upon on the part of the inventor.

The injustice of the present ruling is apparent when one reflects that it is often impossible to get from the office, claims co-extensive with one's invention, and that there are penalties for delay in the prosecution of an application.

Respectfully,

G. H. STOCKBRIDGE.

PHILADELPHIA, PA., December 21st, 1887.

It was my intention to have spoken at the meeting last night on the patent question, but I was so engrossed with what others were saying, that I had forgotten that time was rapidly passing, and before I realized it, it was too late. As I have had some fifteen years experience in patent matters, not only as attorney, but as inventor also, (having at present about 3000 claims of my own pending before the electrical division alone), I write you the following, as I understand you will publish the letters, and my views may be of some use in remedying the present existing evil. I believe the entire defect lies in the Patent Office. It is seldom inventors obtain what they are entitled to, because, they are denied their rights by incompetent examiners who have no capacity for giving judicial decisions. I have been trying for years to get some of my numerous inventions protected by patents, but it is a difficult task. In one instance, I have been prosecuting a new and original invention for nine years and have not, as yet, been able to make the examiner appreciate the invention or grant me adequate claims for its protection. It has been before numerous principal examiners, all of whom have ruled in radically different manners. In another room, it took me six months in arguing a case to convince an examiner of one of the simplest laws in physics. In another case, which I appealed to the Commissioner, I have waited several years without getting a decision, the case having been before three Commissioners, and avoided by each. In another instance I have waited just about one year for a decision in an interference case which should have been given within a week at least after the argument. My experience is that the examiners are not qualified and never will be to such an extent as to invest in them the powers of judicial decisions.

My idea of the Patent system in the Patent Office should be as follows:

The present practice is good in its intents and purposes and is the best in the world. The examination should never be abolished. I have thought of many plans, but the following is the one I deem most fair and devoid of objectionable features.

An inventor (not promoter) must prepare his application with all the care with which it is now prepared, and in all material respects the same. The application or first government fee to be

$5 instead of $15. The application is then examined upon its merits, as is now done; but with more care on the part of the examiners, who must cite *all* the pertinent references they can. The applicant can then amend, if he thinks proper, and ultimately the application is passed to issue. The final fee should not be over $5 to print the patent, *and all of the reference patents, etc. are to be printed upon the face of the patent so issued.* By this means any one buying the patent can see for himself the scope of it, and can use his own judgment. The inventor or his attorney could take any claim they thought right and eliminate or amend only those upon which there is no doubt of anticipation. *The examiners would have no option in the matter*, as should be, for in most cases they are incompetent to judge; but any inventor taking too much is liable to make his patent invalid in part or whole; and if the former, he would lose costs in any suit, as is now the case with any invalid claim sued upon. The courts would then be the only judge between the public and the patentee or owner of the patent. The interference proceedings would take place to prove priority of invention when two applications claiming the same invention were pending at the same time, and the time consumed in an examination would give a distant inventor every opportunity for a fair chance to prove priority. Where a patent has been issued, and another inventor claims the same thing, he may obtain his patent if he so insists, but it will have the reference of the other or prior patent upon its face, and by this means the contest will be carried to the courts once and for all. *No fair law can exist where the first inventor can lose his rights by circumstances over which he has no control.* By this system we would have all inventions published and properly placed on record for future generations, and the arts could not become lost. Putting this system into effect would not require many changes in the present system of the Patent Office. The appeals would remain the same, except that the appeals on the merits of an *ex parte* application would not exist. The examiners would be required to do all that they are really capable of doing, and that is finding the references. They should have no judicial powers on the merits of an application other than to clearly express their opinion as a matter of record in the files.

I think such a system is about as near perfect as it could be made.

Mr. Steuart's proposition to allow the owners of a patent to re-issue without the consent of the inventor is fundamentally wrong, because in many instances the applicant, to elucidate his own invention, often shows other inventors' inventions without making any attempt at claiming them. If the owners were allowed to re-issue without the consent of the inventor, they would undoubtedly take advantage of this state of affairs. As to foreign patents limiting the life of the American patents, I am of the opinion that it was always intended, and should be so construed that there will be no limitation if the American patent is *applied for* before the application for the foreign patent.

I am in favor of a suitable Patent Court, but one which should only act on cases in matters involving interference questions and suits appealed from the Circuit Courts of the United States. I am strongly opposed to any system which will compel an inventor to take appeals to obtain that which he knows to be new, and which he has invented. Put the whole risk upon him and his attorney, with a full record of the patents showing the art printed upon the face of the patent, and I think that little trouble will arise from claiming too much, and nearly all of the trouble of claiming too little will be removed. The Supreme Court and the Commissioner of Patents should be relieved entirely from judicial decisions upon questions of the merits of an application, and the examiners-in-chief could well be abolished if such a Patent Court existed with the removal of judicial powers from the primary examiners.

Very truly,

R. M. Hunter.

New York, Dec. 24th, 1887.

In response to your request for an expression of opinion on the "Revision of the Patent Law," I must confess that the reform of the patent system has not impressed itself upon my mind as one of the crying needs of the day. Certain sections of the Patent Law, such as sec. 4887 on the limitation of the terms of U. S. Patents by Foreign Patents previously granted, and sec. 4916 on re-issues, have perhaps seemed to work some injustice in individual cases, but they are the exceptions, I think, and no human-made law is so perfect as to work absolute justice for all. I have read criticisms from time to time on supposed defects in the U. S. Patent Law, as it stands to-day, but none of those criticisms has made it very clear how the law is to be bettered in any vital respect. For my part, I do not recognize the urgency for, or the advisability of tinkering at the statutes.

With regard to the proposed "Bill to Establish a Commission to Revise the Patent Laws of the United States," I have not, for the above reasons, given the subject very serious consideration. I may say, however, that neither the necessity for, nor the usefulness of such a commission has commended itself to me.

In all probability the members appointed on this Commission would be men who would come to their task, more or less ignorant of Patent Law, and the little they would be able to learn of the niceties of that law during their brief term of office would not make their proposed revision of it of great value.

Whatever their report and whatever their proposed bill might be, however, the whole subject would have to be thrashed over again before the Committees on Patents of the House and Senate.

Why should not those who are to edify the Commission, submit their views to the Committees on Patents of the Senate and House, in the first place, since those committees exist for the same purpose as that for which the Commission would exist? The proposed Commission therefore would only be an incumbrance (expensive in both time and money) in the way of reform, if reform is needed.

Truly yours,
Hubert Howson,
of Howson & Howson.

New York, Dec. 22, 1887.

I enclose a summary and dissection of the various amendments to the Patent Law, first submitted to the Senate and House Committees of the 48th Congress, together with the authorities for the several amendments. This plan of dissection has been found convenient to those members of the Committee who have not time for critical consideration and comparison of the several provisions of the proposed amendments.

A most interesting point, which was in fact the essence of the "Gramme case," which did not obtain presentation at the hearing before Judge Blatchford, but upon which he subsequently consented to a re-hearing of the case, is this. It was determined that the United States Patent was void, having expired with the "earliest foreign patent," which was the Austrian, a "secret patent," so-called, and counsel contended that *because the patent was secret*, that therefore it was not "patent," or due notice or publication or anticipation as to the United States inventor. But in this case the inventor was the same, and the secrecy of the Austrian patent was not material. This theory *so nearly approximated the real merits* as to obscure them, the fact being that the so-called secret patent of Austria is *not a patent at all* in the sense contemplated by the American law. It is merely a caveat, and until a patent is published under the forms of the law, it has no other operation or force than a caveat, and if never published, it never acquires the characteristics of a patent.

Accordingly, there was never, during the life of the Austrian Gramme patent, a moment when Gramme could have stopped the manufacture of his machines, or collected royalty or damage from any manufacturer, any more than the American inventor could obtain such protection for an invention as to which caveat had been filed, but a patent never taken.

The mere labeling of a foreign caveat "patent" or "brevet," ought not to invest it in American law with qualities which it does not in fact possess.

Yours faithfully,
Rowland R. Hazard.

Mr. Frank R. Harding presented a very earnest tribute to the importance of the patent system in which he argued that the future welfare of the country depended in a great measure upon the perfection of our patent system. That there should be a separate department of patents because the system was self-supporting; that the Commissioner, Assistant Commissioner, primary and assistant examiners and examiners-in-chief should hold life positions.

He criticised the faulty classification in the office, claiming that unnecessary and serious delays occur by reason of a case being first placed in a wrong division where it may be pigeon-holed for six months, and is examined in its regular course only to be transferred to another division where it is again delayed.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, March, 1888. No. 6.

SPECIAL MEETING.

The twenty-second meeting of the Institute was held at the House of the American Society of Civil Engineers, No. 127 East 23rd Street, New York City, January 10, 1888, and was called to order at 8 P. M. by the President of the Institute, Mr. T. C. Martin.

The President: The subject which we have before us this evening is of a very interesting nature. The paper is by Mr. Willard E. Case, of Auburn, N. Y., and is entitled, "Electrical Energy from Carbon without Heat." In the absence of Mr. Case, it will be read by Mr. Mailloux.

ELECTRIC ENERGY FROM CARBON WITHOUT HEAT.

BY WILLARD E. CASE.

The following experiments, undertaken by the author, may be of interest as indicating the way in which the cheap generation of electrical energy may possibly be brought about.

In the first experiment, an element was formed through which heat energy was converted into electrical energy, in which the correlation of forces is beautifully illustrated.

It is a sort of voltaic battery, in which plates of tin and platinum, forming the electrodes, are immersed in a solution of chromic chloride, which has no action on the plates at ordinary temperature, so no current is generated. The cell is hermetically sealed, and when heated the liquid becomes active, and part of one of its elements, chlorine, leaves the chromic chloride, goes over and temporarily combines with the tin, forming a proto-chloride of tin. The chemical action generates an electric current, but soon the tin is all converted into chloride, and the current ceases. When the cell is cooled, this temporary combination of the chlorine and tin is broken up, and the chlorine returns to the chromium proto-chloride. The tin being set at liberty, falls as a metallic precipitate to the bottom of the cell in the form of crystals, ready to renew the combination when the cell is again heated. None of the materials of the element are destroyed, they last an indefinite time. The chlorine changes from chromic chloride to tin and back, as often as the cell is heated and cooled.[1]

It is well known that the voltaic battery converts the potential energy of a metal directly into electricity, without heat. This cell, similar to the voltaic battery, converts heat into electricity. The voltaic battery acts at ordinary temperatures, giving up all the energy stored in the metal, and there is the end of it; while this cell acts only when heat is applied to it, converting part of this heat into electricity. After the tin has all been converted into chloride the cell becomes inert, no matter how much more heat be applied. The cell must then be cooled to get this tin back into the metallic state, so here it is necessary to have a difference of temperatures, the tin and liquid being simply a medium by which heat is transformed into electric energy.

It will probably be found that the solution of the tin absorbs heat, and so tends to cool the liquid, and the precipitation develops heat, and so tends to warm the liquid, and that the part where the solution occurs must be kept warmer than that where the tin precipitates. An investigation would probably show that when electric currents are generated the heat absorbed by the solution of the tin is in excess of that generated by the precipitation by the equivalent of the electrical energy developed, and that the possible excess is governed by the second law of thermodynamics. If the cell works between 80° and 180° F., the E. M. F. at the higher temperature is about 0.26 volt, the efficiency is less than 16 per cent., as the possible efficiency $= \frac{T - t}{T}$ reckoned from absolute zero, or $\frac{638^\circ - 538}{638^\circ} = .157$.

In fact, this cell is a heat engine, analogous to the thermopile which is said to convert only 2 per cent. of the energy of the coal into electric energy. In practice probably nothing like 16 per cent. could be utilized.

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[1] This cell has been described in Proc. Roy. Soc. No. 244, 1886.

It is an inexorable law of nature, that under the conditions in which we live a great waste must accompany the transformation of heat into any other form of energy. In the condensing steam engine it requires to condense the steam four or five pounds of water to every pound of steam. Three-fourths the heat used goes to warm that water, and is wasted. In hot air engines or gas engines a cold water jacket must be used, and to it goes the larger share of the heat employed. In the thermopile one set of junctions must be kept cool by circulation of air or water. In Edison's pyromagnetic generator the iron tubes must be cooled by a blast of cold air.

By the second law of thermo-dynamics the minimum amount of heat that goes to this cooling agent is the fraction $\frac{t}{T}$ of the total amount employed, where t is the temperature of the cooler, reckoned from the absolute zero, and T the higher temperature of the working substance; t cannot be less than about 500°, so the numerator of the fraction is always large, and the heat wasted is the larger portion. Understand that this is a law of nature; it is inevitable under the conditions in which we live. No cunningly devised furnace, or feed water heater, or cut-off, or triple expansion apparatus, or pyro-generator can save this heat. The most that any of these devices can do is to save what would otherwise be wasted over and above the proportion $\frac{t}{T}$. But are we to go on wasting all this energy of fuel? Cannot some means be employed to utilize it? We know that the voltaic battery is not a case of the transformation of heat into electrical energy; it produces electrical energy directly. The second law of thermodynamics does not apply, as no heat appears. If we could convert the chemical energy of coal and oxygen into electric energy, directly and cheaply, we would do away with all our steam motors at once. There is no known reason why a cheap substance may not be found which will act on coal and develop electric currents in place of heat. This electric energy would be the equivalent of the heat energy that would be developed by the combustion of the same coal in the ordinary way, and could be transformed into mechanical power, heat, light, etc., with small loss.

So far, little progress has been made in this direction. Some time ago, Jablochkoff produced a battery in which plates of carbon and iron were immersed in fused nitre. The carbon being oxidized, furnished strong currents. The objection to this element is the generation of heat through local action. To it the second law does not apply. Another experiment of more interest than the first, is an illustration of an element which is not affected by the second law, as heat is not essential to its operation. The energy due to the complete oxidation of carbon is converted into electric energy directly.

In a glass cell containing sulphuric acid, C. P. Sp. Gr. 1.81, temp. 75 Fah., two electrodes were immersed, one of platinum, the other of lump graphite; only a slight E. M. F. was indicated 0.007 volt, due to the combination, the graphite acting as a positive element; on the addition of a small quantity of chlorate potassium to the acid, the E. M. F. immediately rose to 0.8 volt, the graphite being disintegrated after a time. This cell polarized rapidly, which was partially prevented by mechanical means. The reaction of chlorate potassium with sulphuric acid may be represented by the following equation: 3 $KClO_3$ (chlorate of potassium) + $2H_2SO_4$ (sulphuric acid) = 2 Clo_2 (peroxide of chlorine) + $KClO_4$ (perchlorate potassium) + $2KHSO_4$ (acid sulphate of potassium) + H_2O (water).

A method of exclusion was adopted to ascertain the oxidant of this electrolyte; chlorine peroxide (ClO_2) appeared to be the only active agent. It is decomposed by the carbon, chlorine being evolved with some oxygen. It was assumed that in this cell graphic acid ($C_{11}H_4O_5$) was formed[1] as the result of the chemical actions. There is probably much waste of energy through local action, as the chemical reactions go on when the circuit is open, but to a less extent. In another experiment an electrolyte was formed of sulphuric acid and chlorine peroxide, the gas being formed in a retort by the action of sulphuric acid on chlorate potassium, and conducted over into the acid in which it was dissolved. This cell, with a positive element of graphite, opposed to platinum, gave an E. M. F. of 0.7 volt.[2]

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[1] See Q. J. Chem. Soc. Vol. XII, 1859.

[2] Great care must be taken in the preparation of this gas, as it explodes at a temperature of about 140° F. It can be preserved in the dark. And is decomposed by sunlight into its component ports.

A similiar combination was used with a solution of chlorine peroxide in water as the electrolyte. The E. M. F. assigned to these cells is only approximate, as it was found dependent on the quantity of chlorine peroxide in solution, which was constantly changing. The resistance also varied with the different degrees of concentration of the exciting fluid.

Different forms of amorphous carbon were substituted in place of graphite in the first form of cell. Gas carbon gave an E. M. F. 0.007 volt when opposed to platinum in sulphuric acid; on the addition of chlorate potassium the E. M. F. rose to 0.5 volt.

Carbon, produced by the action of sulphuric acid on cane sugar, in this experiment, gave an E. M. F. of 0.3 volt.

The carbon of animal charcoal, wood charcoal, coke and anthracite, gave an E. M. F. variable with each form, ranging from 0.3 volt to 1.25 volt. The measurements of E. M. F. of the various combinations were made with the electrode immersed in separate parts of the same solution, that the concentration of the liquid and quantity of gas in solution might be as nearly equal in each cell as possible. It was found when the carbon in comminuted form was contained in a porous cup, that the combination gave an E. M. F. of 1.24 volt, apparently due to the presence of the oxygen of the air.

It is impossible at the present stage of the investigation to assign any definite value of the E. M. F. to these forms of amorphous carbon. In some cases they appear to have an E. M. F. higher than that of graphite.

The energy of the combination of carbon and oxygen, when completely oxidized to (CO_2) carbonic acid gas, is stated to be 9624, foot-pounds per grain equivalent, which is 2.0594 equivolts.[1] In these carbon elements the E. M. F. is not so high, as there must be deducted the counter E. M. F. or polarization, and the energy due to the force of setting chlorine free.

A resort to chemical analysis, of the products formed by the reaction in the cell, indicates the following conclusions:

First.—Carbon produced by the action of sulphuric acid on cane sugar proved to be partially oxidized, or impure carbon.

Analysis gave the following values:

C	62.20 per cent.
H	1.61 per cent.
O	36.19 per cent.

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[1] See Sprague's Electricity, p. 5, 19.

Analysis proves that (H_2SO_4 + $KClO_3$) sulphuric acid + chlorate of potassium oxidizes carbonaceous bodies, like the above product of (H_2SO_4) sulphuric acid on cane sugar, to CO_2, and further, that such bodies first are changed to compounds soluble in the acid (caramel-like substances), which are rapidly oxidized to CO_2.

Second.—Battery carbon of the composition:

C	96.20 per cent.
H_2 O	70 per cent.
Residue of Fe. and Sio_2	2.30 per cent.

The fact was shown that this form of carbon can be oxidized CO_2.

Third.—Wood charcoal, of the following composition after ignition.

C	94. per cent.
H_2 O	5.5 per cent.
Silicious res.	.5 per cent.

This, after repeated re-ignition, was oxidized by the electrolyte to CO_2, no intermediate compounds being formed, and further, in case of pure amorphous carbon the oxidation is complete; that is, that all the carbon is oxidized to CO_2.

These analyses were made with the solution at a temperature of 122 F. to hasten the action, the carbon being in contact with the platinum. In these elements the oxidizing material is too expensive for practical use.

Undoubtedly the direction of experiments in the future will be to find some cheap substance which will absorb oxygen from the air and give it up to the carbon; in fact, acting as a carrier of oxygen, so oxydizing it without heat; and this is not improbable, as we already know of substances which do this, though giving a low E. M. F.; thus, for instance, the ferric salts are reduced to ferrous by agitating their solutions with carbon, being regenerated by absorbing oxygen from the air.

By pursuing this line of investigation, we can be sure we are not ignorantly striving against any law of nature when attempting to convert the whole potential energy of carbon into electrical energy.

DISCUSSION.

Mr. C. O. Mailloux: Mr. Case also favored us by sending a sample of the battery, which you have before you. It consists of a porous cup, in which the graphite in lumps is placed, and outside of which is a sheet of platinum. Sulphuric acid, of the greatest density obtainable, chemically pure, is placed in both the inner and outer vessel. As explained in the paper, in order to render the sulphuric acid active, he puts the chlorate of potassium into the outer vessel, which gives it that light wine color. I have brought a little galvanoscope which I have calibrated for the purpose, so that it gives a rough indication of the voltage of the battery. Each division represents $\frac{5}{100}$ of a volt., and when I close the circuit you will see that it gives a deflection of about 14 degrees. That would be equivalent to about .7 of a volt. I think that Mr. Case states that the electromotive force of his battery is between .7 and of a .8 volt; but, unfortunately, as you notice, the electromotive force falls down very rapidly, owing to polarization. You will notice it is falling down. It is now only 12 degrees. It seems to stay there better, but if I let it rest a little while, it comes to more nearly the full strength, though not quite. I might add for the information of the members that Mr. Case states, in a note sent with the battery, that the internal resistance is about 2.7 ohms, and that when placed on short circuit it gives, approximately, a quarter of an ampère for a short time. He adds, by way of explanation: "You understand I am not showing a practical invention; simply showing a new principle which may be developed." I think you will all agree that undoubtedly there is here a wonderfully interesting principle and one which would be as valuable as the philosopher's stone, if ever even half of what was expected from the philosopher's stone is realized from it.

Mr. Townsend Wolcott:—I would like to ask why it polarizes so. As I understand, the oxidizing liquid is in contact with the platinum. It ought to act, I should think, like the chromic acid in a Poggendorff cell, to oxidize the hydrogen; or, is it hydrogen that polarizes it?

Mr. Mailloux:—He states that the polarization can be prevented by mechanical means; so that it is probably due to uncombined chlorine.

MR. WETZLER:—I was about to suggest that the polarizing might be brought about by the setting free of the peroxides of chlorine; that is the gas produced, and it is probably the one that does the polarization.

MR. F. B. CROCKER:—In regard to this polarization, it is very evident that the active material is peroxide of chlorine ClO_2 following Mr. Case's nomenclature, which is in solution in the liquid. The oxygen of the ClO_2 combines with the carbon, and sets free chlorine on the platinum plate, and that, of course, polarizes the cell. I should imagine that Mr. Case's mechanical means for preventing polarization, are just the same as the mechanical means usually employed in cells which polarize by the setting free of gas on the negative plate, viz.: simply shaking them, roughening the surface of the plates, using a platinized silver plate or anything of that sort, to more or less get rid of the gas, and in that way reduce polarization. I do not see any other cause of polarization there except, perhaps, that there is also the formation of carbonic acid gas on the positive plate. That also would produce polarization.

MR. C. S. BRADLEY:—Perhaps I might be able to give you something interesting. Of course many of you are aware of this method of calculating the possible volt to be obtained from any combination. Now, this seems to me hard to understand. (Referring to equations on the blackboard.) In calculating the volts, I use the whole number; I do not use the equivalents. I use English measure, also. If anybody wishes to reduce it to French measure, or anything of that sort, he can do so. The heat units which are given out by the combination of different substances, of course, can be found in many works. In Watt's Chemical Dictionary, which I have here, you will find the heat of chemical combination and the heat of chemical action. You will find the various energies given and the various heat units given out by an atom of the various substances, and it is very fully carried out and the whole thing has been demonstrated by experiment. Now, we find, when we refer to carbon and oxygen, that carbon, in combining to form carbonic acid—12 pounds of carbon uniting with oxygen will heat 94,800 pounds of water one degree Fahrenheit. That is Andrew's determination. Favre and Silberman give 96,960. Now, in the first place it is necessary to reduce everything to a hydrogen basis. We find that there are two atoms of oxygen. This calls for four atoms of hydrogen. Therefore it is necessary to divide 96,960 by 4, giving 24,240. Now, the volt is about 23,030. I have tried it both experimentally and by calculation, and that is as near as I have been able to come to it. I presume that is not accurate. I do not recommend it as an accurate thing. That, you see, would give a trifle over a volt. It is, perhap, .01 over a volt that that carbon is capable of giving theoretically. That, of course, can be determined experimentally. There is also a way of calculating it from the foot pounds and the correlation of forces, etc. You can get but a certain number of volts out of any combination of two substances, no matter what they are, and you can calculate to just the possible amount that you can get in volts, and you can get no more.

The question of resistance is one of the main points in the whole of this subject; I tried, in 1882, quite a long series of experiments with fused salts and their action upon coal. Of course the oxygen of the atmosphere is absolutely necessary. We cannot, by any means, expect to accomplish direct conversion unless we use the oxygen of the atmosphere. Otherwise, we must use a manufactured product which must be very expensive. Therefore, we have got to introduce the oxygen of the atmosphere, which is just as essential as the coal. That battery costs a great deal more to run from the chlorate of potash used there than a zinc battery. So that to get cheap power in that way, it is necessary that you should use carbon or hydrogen, or a combination of them, and the oxygen of the atmosphere; and to use the oxygen of the atmosphere, you must either supply it at the time you are operating with the coal, or from the manufactured article. Manufactured articles are dear; so it is necessary to use it at the time. Therefore, I hit upon the plan of taking manganate of sodium or potassium and putting a blast of air through the electrolyte, and by that means supplying it with air and allowing it to act on the coal which is put into another part of the vessel. The main point is the resistance. The voltage falls very little short of the theoretical. That is all right; but the resistance is too high for it to be practical. At least, I have always so regarded it. It would require a pond about ten feet in diameter per horse-power. (Laughter.) That is, if you allow two inches between the coal. Of course, coal is a rough substance. You have got to allow a considerable distance between your plate on which the oxygen supply is furnished and the coal, which of course runs up the resistance.

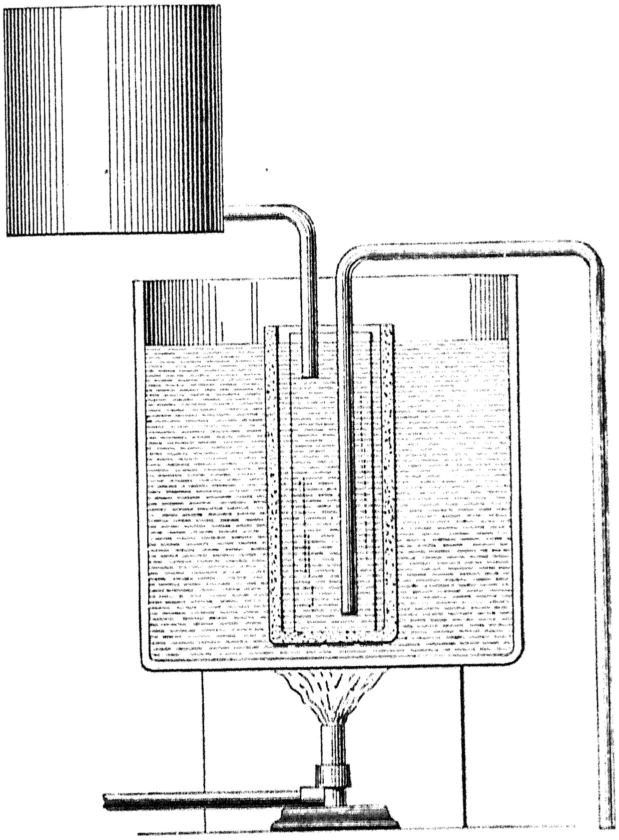
Then, in studying up the matter, I found in Watt's Chemical Dictionary that F. Braun had a determination of the conductivity of fused salts in the neighborhood of their melting points. Putting the conductivity of mercury at zero, that of the best sulphuric acid, is about 7,700, the following numbers were also obtained: Chloride of lead, 32,200; nitrate of soda, 11,475, and so on. But, of all the records I have ever found, and in all the experiments I have tried, chloride of lead is of the lowest resistance or the highest conductivity among the salts. It is about four times as good as the best sulphuric acid. We noticed in some of the electrical papers of a late date, mention of Mr. Acheson's device, in which he proposes to use gases in one case. Now, gases are of a great deal higher resistance than any fused electrolyte, and the fused electrolyte, of course, is, as I said before, too high. The lead chloride is the lowest. It is possible, of course, that somebody may find substances of very low resistance, but it is rather a hard job. Now, of course, I do not mean to advance this as a pessimistic view of the matter at all. It is just simply to make a statement of the conclusions which I have reached which I hope may be of benefit to other people, and perhaps save them some experiments and time.

MR. JOSEPH WETZLER:—The previous speaker, in criticizing the apparatus devised by Mr. Acheson in which gases are used as the medium between the electrodes, has remarked that they are of very high resistance. I think that is a question of relativeness merely. I do not know what the absolute resistance of the fused products is, which the previous speaker employed, but we all remember the experiments originally made by Faraday, who, after placing the points of an electric circuit apart, heated up the terminals of the platinum wires and effected the passage of the current thereby. You also know that if we heat the air sufficiently as is done in an electric arc, we get comparatively very low resistance.

MR. BRADLEY:—I also made this experiment. I employed an outer vessel of zinc in which was placed a porous cup containing a bar of zinc. The outer vessel was filled with a concentrated solution of zinc chloride, and the porous cup was fed by a constant stream of fresh water to maintain the solution within it dilute. The vessel was then heated by a Bunsen burner from below. (See illustration.)

In that way I could control it so as to keep the inner zinc cool, while the outside dish was heated.

It is well-known that chloride of zinc has a high heat of solution. My idea was that if I could keep a strong solution in the outer vessel just on the point of crystallizing in contact with the zinc, and a weak solution within the porous cup, I could cause a passage of the molecules of zinc across, and a current to be generated. The calculated electromotive force from the arrangement was .3 of a volt. I worked at it a good while before I got .3 volt, but I finally succeeded. The question of resistance, of course, comes in and that is the greatest difficulty in all apparatus of this kind. On weighing the outer vessel and the interior plate, the latter should have lost, and the former should have



Bradley's Experimental Battery.

gained. But I found that the inside one had lost, while it was hard work to find that the exterior one had gained anything. So it left me in doubt whether those .3 volts were actually due to the heat of solution or not. But it is quite interesting as an experiment.

Mr. Crocker: That question of equi-volts is rather an interesting one. I shall not go into the figures, but I think Mr. Sprague, of England, has done the subject a good deal of harm by prefixing that "equi" to the well known term "volt." People are used to that word, but the result of prefixing "equi" to it has been that they have let it alone, just as if he had attached a dynamite bomb to it. And really I do not see why it is put there. An equi-volt, as far as I can see is just as much a volt as any other volt. It is well enough, perhaps, to designate volts obtained by chemical action as equi-volts; but I do not know any reason for it. You might just as well separate volts obtained by an Edison dynamo and call them Edi-volts. Really these equi-volts are just plain, ordinary North American volts. And I think that if the calculation of the E. M. F. of chemical action was more often used it would be of great service in a great many cases. For example, Mr. Bradley and I, before we had made any experiment whatever on the subject, calculated out what would be the electromotive force of a combination of zinc and bromine; and we calculated, just as Mr. Bradley described there, by taking the heat units produced by the combination of zinc and bromine and then going through those figures we got 1.8 volts. By a great many experiments very carefully carried out we got 1.78 volts. Now that is just .02 different from our calculations. At first we got 1.8, because, of course, the instrument would not detect that .02. It was only by continual experiments that we got the average of 1.78. Now that is close enough for all practical purposes, and you can arrive at results which are practically close enough, with a combination of zinc and iodine, for example, or any combination of two elements, or an element and a compound body. For instance, you can calculate out the electromotive force of zinc and chromic acid, or nitrate of soda or anything of that sort, and you will arrive at results which approximate plenty close enough for practical purposes with the actual electromotive force obtained by experiment.

Now, that seems to me to be a very important thing, because very often you want to find out these facts and have not time to try them and you save yourself the experiment by calculation. Of course all kinds of short cuts like that are very often a great advantage to the investigator, and so far as I know this one is very seldom used. Mr. Sprague is about the only authority who speaks of it at all, and in practice I have never met any one who used this method. Some people doubt that you can get reliable results in this way. They claim that they are mere accidental coincidences, but I think when they occur a great many times and when you can actually prove mathematically, that there is such a relation, then there can be no longer any doubt about it.

The contact theory has also prevented the adoption of this calculation of electromotive forces. The idea is that there is a contact difference of potential; that it is not due to the chemical action, but is due to the contact; the chemical action merely keeps up the action. I never could see much in that contact theory myself. If you have a certain amount of chemical affinity and it produces so many units of energy in the form of heat in combustion, why shouldn't it produce the same amount of energy in the form of electricity in a voltaic cell? The contact theory is one of those things that you cannot see any reason for—why should zinc and bromine have a contact difference of potential of 1.8 volts? Whereas you can see a very good reason why they should have a chemical electromotive force of that amount. The contact theory is a purely arbitrary thing. A man tells you there is a contact difference of so and so. There is no reason for it that I can see, and the fact that the contact difference of potential is usually the same in liquids as in air, is due to the fact, that there is oxygen, moisture, etc., present in both cases, and you have substantially the same conditions in the air that you have in a voltaic cell.

Those two theories of contact difference of potential and the "equi" prefix to "volt" have retarded a very important branch of science, I think.

Mr. Wolcott:—I do not understand why the zinc would be dissolved off from one electrode and deposited on the other. Does the chloride of zinc dissolve the metallic zinc?

Mr. Bradley:—The Grotthuss hypothesis is generally understood—that there is a chain of molecules of chlorine and zinc, all the way across the electrolyte. If there is more affinity for the zinc on one side than there is on the other, the zinc will cross over as soon as the circuit is closed.

Mr. Wolcott:—But I do not see where you get your affinity unless there is some tendency of the chloride of zinc to attack the zinc.

Mr. Bradley:—Here the water plays a part. Of course you could devise all sorts of theories to account for it. The water, of course, and the zinc chloride have an affinity for each other.

Mr. Wolcott:—If you have free chlorine you are all right.

Mr. Bradley:—If you have got free chlorine on one side and zinc on the other; here you have free chlorine on one side and free water on the other.

Mr. Wolcott:—You have got something to attack that zinc to begin with.

Mr. Bradley:—Not necessarily. Suppose you are transferring copper by electrolysis.

Mr. Wolcott:—You have got the outside electromotive force to do that.

Mr. Bradley:—Here you have got a chemical affinity or a chemical pressure.

Mr. Wolcott:—You have got a chemical affinity, but it leaves out the zinc.

Mr. Bradley:—It is the heat here that is doing it.

Mr. Crocker:—The chloride of zinc does dissolve the zinc off on one side.

Mr. Bradley:—Yes, and deposits it on the other side.

Mr. Crocker:—Just the same as in the case of chlorine and zinc cell. You do not have the chlorine directly in contact with the zinc. You have chloride of zinc between.

Mr. Bradley:—The chlorides of tin are the best to perform it with. In this case you have the stannous chloride on one side and the stannic chloride on the other, and then the chlorine runs across.

Dr. Grimshaw:—I should like to inquire, in reference to the allusion there to the graphitic acid, if that alleged fourth condition of carbon called *graphon*, has been positively proved to exist?

Mr. Mailloux:—I can only say that the paper was handed to me but a few minutes before the meeting began, and I must take it from Mr. Case's own statement. I can neither substantiate it nor refute it. However, as the presence of other, similar, analogous acids has been either suspected or proved, such as silicon into silicic acid and various others—phosphorous, for instance, and sulphur, itself an amorphous substance; while on the other hand we have plumbic, cupric and boric acids and various other acids of that character, and it does not seem to me impossible that graphitic acid could exist. As I say, I only take it on hypothesis, which is the way Mr. Case gives it to us.

DR. GRIMSHAW:—There have been cases where the existence of new compounds of that kind has been announced and the statement afterwards withdrawn. There was one time when gas-carbon was suppposed to be a fourth amorphous condition of carbon, and then the name *graphon* was applied to it, as a body forming an acid of that character. Afterwards I thought there was some doubt about it. A good many of the conditions of these elements ending in "on" have been shaken up badly within a few years. For instance, the amorphous condition of boron has been decided, I believe, to be the boride of aluminium. Very often it is a question whether alleged forms of carbon are not boron instead of carbon. The silicic acid, to which Mr. Mailloux referred, used to be known as the teroxide. I believe that is now upset. The amorphous forms of these three elements are pretty badly mixed in the minds of chemists.

Adjourned.

NOTE ON THE ABOVE DISCUSSION.

BY WILLARD E. CASE.

In answer to the inquiry of Dr. Grimshaw in reference to graphitic acid, I will say that according to Brodie Phil. Tr. 1859, 249, graphite is attacked by such an oxidizing mixture, as H_2SO_4, and $KClo_3$, gradually oxidizing it to $C_{11}O_5H_4$ (graphic acid) a substance crystalline and insoluble in all reagents, while amorphous carbons are oxidized to substances soluble in the strong acid mixture, but reprecipitated by water. As shown by the first analysis amorphous carbon in contact with platinum in a mixture of H_2SO_4 and $KClo_3$ forms (caramel like) substances soluble in the acid which are rapidly oxidized to CO_2. Graphic acid is formed then, only from the action of this mixture on graphite. Berthelot in a very extensive investigation. "Recherches sur les etats du carbon ann. de chim. Phys. [4] XIX—392" verifies Brodie's work, but says the name graphitic oxide is better, as C_{11} H_4 O^5 is not an acid in a strict sense. When amorphous carbons are used as the oxidizible substances, chlorine is liberated on the platinum plate and carbonic acid gas on the positive plate. I adopted the expression "equi-volt" as I preferred to follow the terminology of the authority quoted. The E. M. F. can be calculated as Thompson showed by the first law of thermodynamics in 1851, that $E = Je\theta$, θ being the thermal value of all the chemical reactions going on per gramme of some one substance acted on, when a current is produced by or passed through the cell, e being the electro-chemical equivalent of this same substance J being 42×10^6.*

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* See Jenkin Elect. Mag., 3d Edition, 1876, p. 169-170.

Read before the twenty-third meeting of the Institute, New York, February 14, 1888.

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ALTERNATING CURRENT ELECTRIC MOTORS.

BY DR. LOUIS DUNCAN.

The alternating system of electrical distribution, possessing as it does many advantages for distributing electrical energy over extended areas, possesses certain disadvantages, among others that of not, at present, allowing the use of electric motors for the distribution of power. Let us, before taking up the special subject for consideration to-night, briefly review the general subject of alternating current distribution.

In this system are employed currents of high and constant potential, varying from positive to negative many times a second. If we represent current and potential by curves, whose heights above a horizontal line represent ampères or volts, the horizontal distance representing intervals of time, we will have something like the following: Fig. 1. *II* representing current, *I* representing electromotive force; the maximum value of the current lagging behind the maximum of E. M. F. in a way that you were shown recently by Mr. Stanley.

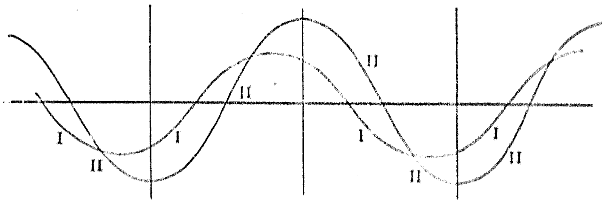

Figure 1.

The high potential used in the dynamo or primary circuit would not be suitable for incandescent lighting and would be dangerous; it is, therefore, reduced to a low potential at points of consumption by "transformers;" that is, induction coils working backward. Now, the whole beauty of the system lies in this; that by using high potentials in the primary circuits we can transmit a great deal of energy with comparatively little current and, therefore, with little loss in the lines. This enables us to use small conductors and to avoid the large investment in copper necessary in distributing energy by the direct system.

In any central station supplying electric lights, the full capacity of the plant is utilized but a short time during the day, and taking the whole twenty-four hours, we will find that we have sold an amount of energy equal to a half or a third—perhaps even less—of the amount we could supply, supposing there was a demand for it all. Let us represent by a curve, Fig. 2, the amount of current used during the day in a constant potential system; the potential being constant, the energy will vary with the current, it being equal to $E\,C$.

Figure 2

The amount of energy we could have sold is represented by the total area $A\ O\ X\ B$; the amount we have sold is the area $D\ E\ F\ G\ H\ I\ X\ O$. If we had been able we would have three times the receipts, with expenses perhaps half again as much as in the first case.

A great problem before electricians is to fill the vacant spaces, $A\ D\ E\ F\ G\ H\ A$ and $H\ I\ B$. There are two ways to partially accomplish this. Suppose, in the first place, we put electric motors on the circuits and sell power during the day. What effect will that have on our diagram? If the motors are at work from 7 A. M. to 6 P. M., we can sell so much power as will at no time make the sum of the power and the energy used for lights greater than the maximum capacity of our station. The area representing the power we can thus sell, is shown by $G\ K\ h\ m$ on the diagram. The total solid part of the diagram is all that an alternating system can supply; the direct system could fill the entire area by using storage batteries.

Our knowledge on the subject of alternating currents is largely due to the work of M. Joubert and Dr. John Hopkinson. The latter showed first that alternating dynamos could be coupled in parallel, and afterward that one dynamo could be used to drive another as a motor. He brought forward experiments tried by Prof. Adams and himself, in which one dynamo had driven another, the latter doing mechanical work. Mr. Siemens at the same time, in 1885, described experiments where he had driven both alternating dynamos and ordinary series motors by alternating currents. Dr. Hopkinson's mathematical work assumes that the curve representing E.M.F. is a simple sine curve; that is, that $E = E_0 \sin 2\pi\frac{t}{T}$; it is important to notice, however, that his principal conclusions do not depend upon this assumption, but would be true if the E. M. F. were any periodic curve. Since that time nothing of importance has appeared on the subject excepting two short papers, one by Mr. Kapp on "The Maximum Work Obtainable from a Given Source of E. M. F.;" the other by Mr. Blakesly on "Conditions of Maximum Efficiency in the Case of Transmission of Power by Alternating Electric Currents."

In all of the mathematical work done on this subject it has been assumed that the various E. M. F.'s may be represented by a sine curve, and that only simple electro-magnetic actions enter into the problem. I do not believe that these assumptions are true. Since Joubert's experiments, made in 1881, it has been known that the curve of E. M. F. of a Siemens alternating dynamo, where the external circuit had no statical capacity, was very nearly a sine curve. With some of the machines used to-day, and under the much more complicated conditions of distribution, this is probably not even approximately true. Neither is it true that the statical actions of the different forms of wire upon each other in the transformers can be neglected. Messrs. Hutchinson, Wilkes and myself have lately experimented on this subject, and we have obtained the curves for the currents in the secondary and primary circuits under many conditions. These observed results do not agree with the results of calculations in the few cases that we have worked out. Until we have fully satisfied ourselves in this matter I will avoid any but the simplest mathematical work.

Alternating current motors are not in successful use; let us try to find out the difficulties they present and attempt to suggest plans for overcoming them.

The energy being transformed at any moment in any dynamo-electric apparatus—in which I include dynamos, motors, transformers, etc.—is equal to the product of the current by the electro-motive force produced by the apparatus. In the dynamo, for example, the amount of mechanical energy being transformed into electrical is $E\,C$, where E is the electromotive force produced by the dynamo; in a motor the electrical work transformed into mechanical is $e\,C$ where e again is the E. M. F. produced by the apparatus, the counter E. M. F. of the motor. This is true for alternating as well as for direct current apparatus. Generally we have:

Work $= E\,C$. This is + for a dynamo − for a motor.

In alternating circuits E and C are constantly changing. If we represent them by curves, as before, we must multiply each value of C by the corresponding value of E, and if we add all

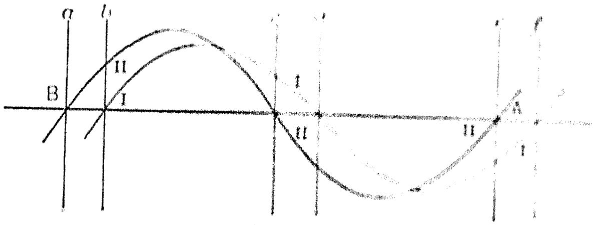

Figure 3.

these together from A to B, Fig. 3, we will have the work done by the current in one period. Now if we consider values above the horizontal + and below it −, we have from c to d a + value of the product, from e to d a − value, from e to b a + value, from b to a −. The total work done then is [c to d] + [e to b] − [d to e] − [b to a].

Now we see that if we shift II forward or pull I back, we have this work less even, though the curves themselves remain the same size. Now, this is one of the exceptions I take to the alternating system. In a direct system as the work decreases the current decreases, and the heating of the mains becomes proportionally less, since it varies as C^2; for example, if the maximum loss in the mains of a direct current plant were 10 per cent. of the average, I imagine that the average loss is not over 3 per cent. In an alternating current plant, a decrease in the consumption of energy decreases the current very little; it simply shifts its position with respect to the E. M. F., and the heating, approximately equal to $\frac{C_0^2}{2} R$, remains nearly constant. A maximum loss of 2.5 per cent. would probably mean an average loss of at least 2 per cent. This same remark applies to transformers and motors, and the average efficiency of the former will be considerably less than the maximum efficiency.

A distinction between continuous and alternating current apparatus is this: In continuous current dynamos and motors the cores making up the windings of the armatures make different angles with the lines of force in the field, and the result, when the armature rotates, is a constant E. M. F. fluctuating but little; in alternating dynamos and motors the alternate coils are symmetrical with respect to the alternate poles, the remaining coils being symmetrical with respect to similar poles of opposite signs. The result is a varying E. M. F., changing as has been shown. In dynamos the effect of this is that the power applied either varies from zero to a maximum each period, or the armature changes its speed at different points and oscillates with a period equal to that of the E. M. F.

Let us now consider what takes place in the case of two motors, one continuous, the other alternating, one between mains where a constant continuous E. M. F. is maintained, the other where there is a constant alternating E. M. F. In the first case representing current and E. M. F. by curves, as we have been doing, we will have two straight lines, Fig. 4, one above, the other below our horizontal axis, the former representing current, the latter the counter E. M. F. of the motor. Reckoning time along the axis we have for the energy transformed in the interval from a to $b = - E C$. As this is $-$ it means that electrical is transformed into mechanical work.

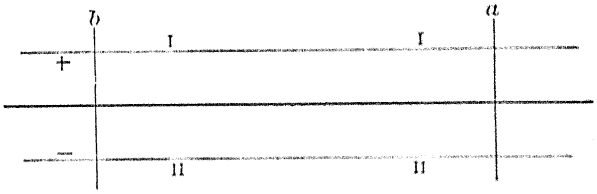

Figure 4.

Now, let us consider an alternating current motor built like an alternating dynamo, the field excited by a continuous current. If *A B C*, Fig. 5, are the poles of the field magnets, supposed to

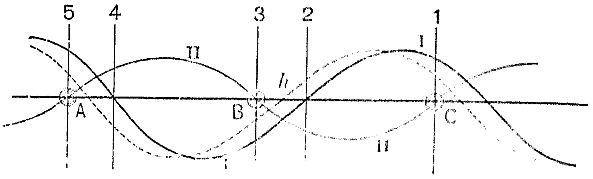

Figure 5.

be excited by a continuous current, then the curve *I I* will represent the counter or motor E. M. F. If our motor is running slowly there will be two or three reversals of current in the time it takes a coil to go from *A* to *B*, Fig. 6, and the product *E C* will be nearly zero, the positive and negative parts being almost equal. Whether it is + or — is somewhat a matter of chance, and if it is — from *A* to *B* it will very likely be + from *B* to *C*. Thus the armature is pushed first in one direction, then in the opposite, and there is no definite tendency for it to rotate as a motor. This brings us to our first difficulty, a simple alternating current motor will not start itself. Let us suppose, however, that it has been started by some means and has reached such a speed that in the time of a reversal a coil shall have moved over the distance between two similar poles. We will have the state of things in Fig. 5, and the armature will continue to rotate. Now the position of the curve *II* is fixed; we will consider the position of *I* as determined by the position of the armature coils when the current in them is zero; for instance, if the speed increases a little the curve will advance as shown by the dotted line. The total work transformed is the product of the two

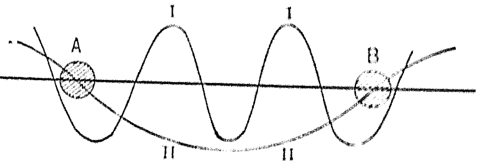

Figure 6.

curves; from 1 to 2 it is —, from 2 to 3 it is +, from 3 to 4 —, from 4 to 5 +. The result is — [1 to 2 + 3 to 4] + [2 to 3 + 4 to 5]: part of the time then the machine is working as a dynamo, part of the time as a motor; the difference of the values represented by the brackets gives the mechanical work that is really available. Now while the available work is the difference of these areas, the difference becoming small as the load is decreased, the heating is the *sum* C^2R for all the values of the current, and is independent of the position of its curve, as has been pointed out above.

Looking at the figure again we see that from 1 to 2 the armature is pushed forward by the current in the line; from 2 to 3 it is pulled back, since it is acting as a dynamo feeding into the line and can only get the energy to produce the current by decreasing its speed and drawing from its energy of motion. The armature oscillates then and it is evident that the amount of its oscillation depends on the kind of work it is doing. If it is driving heavy wheels or machinery having considerable inertia it will only have to slow down slightly when it becomes a dynamo. If it is lifting weights the amount of oscillation will be considerable.

It is evident that there is a certain position of the curve I that will make the available work a maximum. If the motor is doing all of its possible work the curve will take up this position; as the load is decreased the speed will increase for an instant until the curve has shifted forward into such a position that the sum of the products — $E\,C$ is equal to the work done. In fact, we can plot a curve representing values of the distance from the point the current curve crosses the axis to the same point when the work is zero corresponding to different loads.

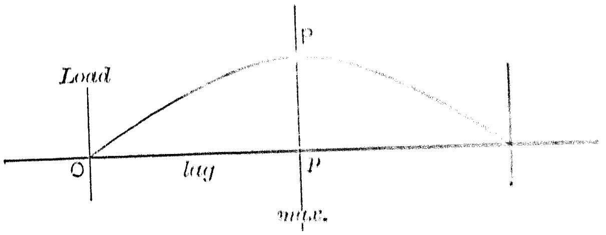

Figure 7.

Now the value of this lag, as we call it, cannot be greater than $O\,P$. Fig. 7, for although we might extend our curve on the other side, yet a slight increase in our load will cause the armature to fall back, decreasing the available work, and suddenly stop. It is, in fact, in unstable equilibrium.

Suppose we have found the value of the lag that will give us a maximum value of the work, and have calculated this value by the ordinary mathematical methods employed, the real work we can obtain is less than this for the current curve oscillates on both sides of the maximum, supposing we *could* work at the maximum; that is, it is only for a very short time in the best position; so that if we take the sum of the work for a period, it will be less than the calculated maximum. In reality we must work slightly in advance of the maximum, for if we were too near it the curve would fall behind *P*, and will then be in unstable equilibrium and stop. The practical maximum will then vary according to the amount of the oscillation; that is, according to the nature of the work being done.

It seems to me that the possible forms of alternating motors are: (1) ordinary series motor; (2) an alternating dynamo reversed, the field being excited by a continuous current; (3) an alternating dynamo reversed, the field being excited by the alternating current first corrected by a commutator on the shaft; (4) the arrangement suggested by Prof. Thomson, which is described below.

In the first type—a series motor—there is no difficulty in starting; the motor will start by itself. There are these troubles,

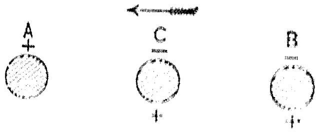

Figure 8.

however: the armature and field must both be thoroughly laminated to prevent eddy currents; the magnetism of such large masses of iron being rapidly reversed, will cause losses unless both field and armature are far from saturation; that is, the mass must be great. Again, Mr. Kapp has shown that to obtain the maximum work we must have, approximately, the counter E. M. F. of the motor equal to the E. M. F. of self-induction—a condition almost impossible to realize in practice. The motor must be governed in some way, as it will not govern itself.

The alternating motor with a field fed from the alternating circuit, the current being commutated, will start itself. I do not know if this has been generally recognized. If, in Fig. 8, *A* and *B* are poles, and *C* one of the armature coils, the effects of the currents will be as shown by the two sets of signs. The effect of either arrangement is to move the armature in the direction of the arrow. When C gets opposite A, the commutator changes the relative directions of the currents in C and A, and C is repelled by A and the armature continues to rotate. The maximum work will, I think, be done when the speed is such that a reversal takes place in a distance A to B. The mathematical investigation is difficult, and until I am satisfied as to the assumptions usually made I will not attempt it. We have as an advantage of this type that it will start itself; a disadvantage is that the fields must be carefully laminated, there is loss in reversing them, and there will be for some speeds considerable sparking at the field commutator. I should not imagine, either, that the work obtainable from such a motor would be as great as if its field were fed by continuous currents.

The motor obtained by reversing an ordinary alternating dynamo has advantages and disadvantages. It perfectly regulates itself and the field magnets need not be laminated; that is, it can be made cheaply. It will give a greater output from a given source of current than corresponding machines of either of the types already discussed. Its disadvantages are that it must be started independently. We must have a continuous current to excite the field, and if a load having any considerable inertia be suddenly applied, the motor will stop. This last objection, by the way, will apply to the type of motor previously discussed, provided, as I have supposed, the maximum work is obtainable when the counter E. M. F. has the same period as the applied E. M. F.

Let us see what means we can employ to start the motor and excite the field. We could start by passing the commutated alternating current through the field as in the second type of motor discussed, changing our connections when the proper speed is attained, so that a continuous current from some external source passes through the field and the alternating current is shut off from it. Another way would be to have on the same shaft with our main motor a motor arrangement such as was described by Prof. Elihu Thomson in his valuable paper on alternating current phenomena, read before the Institute. With this arrangement we can do more than start the motor. Prof. Thomson pointed out that when this auxiliary motor reached a certain speed, it would produce a continuous current in the external circuit of its armature. This current could be used to excite the field of th main motor. By properly proportioning the number of coils i the main and auxiliary motors, this continuous current would b produced just when the motor had arrived at its proper spee and it is evident that we can make this current operate an automatic device to make the circuit of the main motor at this moment. I should imagine, however, that a motor made in this wa would be expensive and not particularly efficient.

What seems to me the simplest, cheapest, and most efficien means of running alternating current motors is this: build th motor on the same general plan as the dynamo, with such modifications as the different conditions of working impose, and start i and excite the field magnets from a continuous current circuit, run with our alternating circuit, supplied with current by a dynamo at the central station. If we wish to distribute 500 h. p. our continuous circuit should have a maximum capacity of about 50 h. p. To start the motor we should have the following arrangement: there should be two breaks in our armature circuit, one between the regular brushes of the machine, the other at a commutator for the continuous current. At the second break the two ends of the circuit should be taken to alternate bars of the commutator, the number of bars being such that the direction of the current is reversed every time a coil passes a pole. The alternate bars are normally connected by a metal ring pressed against them by a spring; in this case we will have the normal circuit just as if there were no continuous current commutator. On the motor would be a switch-board that would accomplish the following things: If we wish to start we turn the handle of the switch to a certain position; this will short circuit our regular brushes and by the aid of a couple of levers will drop the continuous circuit brushes on the commutator, at the same time pulling away our metal ring. The motor will then start as a continuous current motor; when it has reached its proper number of revolutions or is above it, turn the handle a little further; the continuous current brushes will be raised, the metal ring will connect the commutator bars, and the alternating circuit will be made and the motor will continue to run and will do work.

Now, I consider that the advantages of this plan are these: It makes the motor simple and cheap to build; it will be more efficient and have a greater output than the other motors that have been discussed; the currents to excite the fields will have to be supplied anyhow, and they cannot be supplied more economically than from the central station; being simpler there will be less liability of derangement.

The motor described by Professor Thomson that I mentioned above consists of field magnets through which the alternating current flows, with an armature not in any way connected with the external source of current, but which has currents induced in it just as if it were the secondary of a transformer. The circuits of all of the armature coils are open excepting one that is in such a position that the current induced in it will be repelled by the pole-pieces. In this form I do not think the motor will be particularly efficient or give any considerable output. It is possible that the principle may be further developed to give a considerable output, but I do not believe that any motor in which the field is fed by alternating currents can be cheaply made, although this is a matter with which I have no experience.

To prevent motors stopping when the load is thrown on it would only be necessary to drive the load from a friction pulley on the shaft of the motor. The pulley should be so adjusted that it will turn just before the motor falls behind its point of its maximum work.

It is possible that all of the plans I have suggested have been tried by the different inventors who are at work on this subject. If a motor of each type that I have mentioned were built and proper experiments were tried upon them, we would—provided we had the ability to interpret and apply our results—have the data necessary to design the best possible machine of each type, and if we build these it should settle the question as to which form will be the best. There is no necessity in building an indefinite number of motors to experiment with. It is too much the custom, if we wish to design a dynamo for instance, to build twenty different ones and see which is best. One machine, with proper experiments and deductions, should put us on the right track.

Dr. Holmes once said that when he had written anything that pleased him it immediately seemed very old indeed. I, with humility, feel much the same way, as if what I had said this evening is so apparent that every one must have known it. I can only hope that some of the suggestions I have made will help the development and final successful application of the alternating current motor.

DISCUSSION.

MR. TOWNSEND WOLCOTT:—I have been very much interested in Doctor Duncan's paper. It throws rather a different light upon the subject of alternating motors from the paper of Mr. Stanley. From Mr. Stanley's paper we concluded that the alternating system, all the way through, was slightly more economical than the direct. But Doctor Duncan has shown, I think pretty conclusively, that there are various losses there which we do not have in the direct system. The loss from heat there, which depends upon the square of the current, I think, is very neatly demonstrated—the work depending on the product of the current and the electromotive force, changing in sign whenever the electromotive force changes, while the loss in heat is constantly plus, because the square of the minus quantity is plus, as well as of the plus quantity. There is one advantage, I think, in the alternating system which is entirely of a practical nature, irrespective of the efficiency, and that is that you do not use any commutator at all. I know one of the greatest practical difficulties in motors and dynamos, is the trouble of having a commutator. Of course, in favorable conditions of working, the commutator may last a long time without need of repair; but, as I have had reason to observe especially of late, in railroad work, the commutator gets played out pretty quick in running street railways. If there is any possibility of running a street railway with an alternating motor, I think it would be a great advantage even if it was not so economical. Even if it lost twenty five or thirty per cent. of the energy more than the direct motor, I think in some cases it would be preferable.

MR. C. O. MAILLOUX:—I would like to remark with reference to the application of alternating currents to the purposes of electrical traction, to which Mr. Wolcott alludes, that it will probably be some time before we succeed in utilizing it, however great may be the advantage of its use; because the alternating current motor fails to afford one of the chief requirements demanded in a motor, for electrical traction—namely, capacity for producing great torque or effort at the periphery of the armature at the time of starting. This is exactly what the alternating current will not do. Doctor Duncan has pointed out the necessity for gradually putting the load on, so to speak; while as we know, in electrical traction, it is necessary to develop large pulling capacity or effort. There is a great deal of confusion in that respect as to what is really required of a motor. You may rate a motor at a certain power, and it may be capable of giving you that energy when running at a certain stated speed, yet the motor may be for all that, very far from answering the purposes which it must fulfill in traction, because at the time of starting the car, the effort required is more static than anything else, and while in order to measure kinetic energy by the standard of foot-pounds we must have motion and pressure, or feet and pounds—at the time of starting the car we have only pounds and no feet. In other words, it is a static, dead pull, and the motor must be capable of supplying that pull or else it cannot hope to be sucful in propelling a vehicle of any description; and while a motor, as I have just said, might be capable of developing its nominal power, and more than that when running at a given speed such as would propel the car at its rated miles per hour, yet it might prove quite inadequate at the time of starting, and more especially if the car happened to be on a grade or curve, from the very fact that it is not so organized as to be able to develop that effort at the periphery of the armature which is necessary to give the static pull; and that is wherein the alternating current motor does not seem to me to give any hope, at least for the present, and for a considerable portion of the future; because we would have to use machinery, such as friction clutches and various other devices of that kind, so as to gradually apply the work to the motor.

MR. F. B. CROCKER: There are two or three practical points that suggest themselves to me. In the first place, Dr. Duncan refers to the great cost of a laminated field, and he speaks of it as if it were practically a bar to the use of laminated fields. If I were going to make a wrought iron field, or even if I were bidding for a contract to make wrought iron fields in quantities, I should prefer to make them laminated rather than make them of forgings or any other form of wrought iron that I have ever seen used for this purpose—fitting bars together, for instance. So far as I am concerned, I prefer the laminations, and since I have been experimenting a little myself, in this direction, I have been quite pleased with that very feature of the alternating current motors—that they do involve laminated fields.

In regard to the putting on of a load suddenly. That is a difficulty no doubt, and a serious one; but still the load does not go on with the mathematical suddenness that Dr. Duncan assumes.

The belt will slip and stretch more or less and the load will gradually come on. Of course it would be possible to stop a motor, and even bring it to a stand still with a heavy load very suddenly applied— but in ordinary cases where the load is not extreme, it will come on gradually, the motor will take it and I think will run along with it. Of course this is all a question of degree. But I think this difficulty could be overcome more or less. It would be desirable to avoid introducing alternating current motors where they had to suddenly take a heavy load; but that would be, of course, a limitation.

As to the objection that Mr. Mailloux brought up that an alternating motor must obtain its speed to exert any power or pull—it applies only to one particular kind of alternating current motor. Dr. Duncan enumerated four. I do not remember the classification exactly, but I think Mr. Mailloux's objection only applies to one or two of the four classes. The other two would be entirely outside of Mr. Mailloux's objection. Therefore the alternating current, broadly speaking, is still possible for electric traction; and if an alternating motor of either of the other two classes can be made of sufficient power it would start just as well as a continuous current motor. The question is, can it be made of sufficient power? That is a difficulty with alternating current motors; they have not got the power or torque, as Mr. Mailloux says—they have not got the vigor in them that the continuous current motor has, up to the present time at least; and the problem is to increase their effect—make them more powerful with respect to their size than they are present. The same is true, more or less of alternating current dynamos, I believe. They are large and clumsy for their output. But alternating current motors of the two classes that start themselves are still more clumsy and large for their power, and those are the two classes which I look to as being the ones to improve and to get into practical shape, because I think a motor has got to start itself in order to be useful for practical work, and an auxiliary armature or an auxiliary commutator or any other auxiliary apparatus is a very objectionable thing. Of course if you cannot get along without it you have to accept it, but I think it is very desirable to eliminate it.

Mr. Mailloux:—I just want to add a word of comment to Mr. Crocker's remarks; that those alternating motors which would be able to start themselves and which he thinks might be able to develop the torque required are the very ones which are no longer free from the disadvantages of devices for commutating and other troubles of that kind which are incident to the continuous current motor.

Now, as to the production of torque, I think that the objection still holds good, because the trouble of heating as it is related in some way to self-induction—or, at least the production of eddy currents in the mass of the iron itself, even though it be laminated—still applies; and as in order to produce a powerful effort at the periphery of the armature, it is necessary to increase the current very largely, I doubt very much if the curve of torque would be such as you would obtain from a continuous current motor. I fear that it would not rise so quickly and that it would reach the limit very much sooner. Then, besides, I do not see how it would be possible, with the present resources of the art in the present state of its development, to regulate the current, so as to render the use of the motor for that purpose as elastic as the continuous current motor, because, there are a number of limitations or conditions imposed by the current itself which are very hard to overcome.

LIEUTENANT F. J. PATTEN:—Dr. Duncan has shown us that the curve of counter-electromotive force has a variable position with respect to the mean, *i. e.*, oscillating from side to side, and I wish to ask, whether any such oscillation takes place or not, provided the motor is running under a constant load.

DR. DUNCAN:—I think the oscillation will take place no matter what load is on the motor; because in a part of every period—and a period takes place every one three-hundredth of a second—the motor is not a motor at all, it is a dynamo. That is, it is absolutely putting current into the main line. The only way to get the energy to throw that current into the main line is, by decreasing its speed and drawing from its energy of rotation. Therefore, where it is in the positive areas, it will slow down in speed; where it is in the larger negative areas, it will increase its speed. But the oscillation will occur whether the load is constant or not.

LIEUTENANT PATTEN:—The question is fully answered with a single exception regarding the high number of alternations. I would presume that the oscillation referred to is infinitesimal in its character and would make no difference in the practical regulation of the motor in a finite amount of time.

DR. DUNCAN:—If the motor were driving a load which had a considerable inertia—any revolving apparatus which had a considerable inertia compared with the amount of work—the amount of the oscillation would be infinitesimal. But if the motor were light and were lifting a heavy weight, then the amount of oscillation would be very noticeable, and would seriously decrease the maximum work which the motor could do.

Mr. Crocker:—As regards the regulation of motors, and the point which Mr. Mailloux brought out, it happens that one form of alternating current motor that I have been working upon lately is particularly well adapted to being regulated. One of the most desirable means of regulation in any motor can not be used in an ordinary direct current motor, on account of sparking, that is, shifting the brushes. When we regulate a motor by shifting the brushes, it creates very serious trouble there, because we pass off the non-sparking point on to the sparking zone, and that is very objectionable, because that is really the great difficulty, as some of the speakers have said, in all motors and dynamos with the commutators and brushes. It happens that one form of alternating motor works in just the opposite way exactly and as you decrease the power you decrease the sparking, and when you reverse it there is no sparking at all. Therefore you can regulate it in that way, which is just the way every one wants to regulate the motors if he could.

Mr. Mailloux:—How about the torque?

Mr. Crocker:—I admitted before, and I now readmit that small power and torque is the difficulty with alternating current motors; they have not at present got the power of direct current motors, or, at least they apparently have not got the power. But still, dynamos and direct current motors were very weak in their early stages. When there is something to start on it is generally possible to add five per cent. here and five per cent there, and, in this way, gradually coax the result up to a respectable magnitude.

Mr. S. S. Wheeler:—There has been considerable discussion about the torque of an alternating current motor, and Mr. Mailloux prefers the direct current motor for his car, because when the car is going slowly, the motor going slowly will draw a heavier load and will give a tremendous power momentarily. There are two classes of direct current motors, one of which does not increase its torque when the speed is slow, and the other does increase its torque—the constant current and the constant potential motors. The alternating motors which have been described seem to resemble the constant current motors in having a constant torque. I would like to ask Dr. Duncan to give us a little information about what can be done in shifting the proportions or changing the design of alternating current motors to make them resemble more closely the constant potential motors, so that greater torque will be secured when the motor goes slower.

LIEUTENANT PATTEN:—I have designed a form and made some elementary tests of a motor, of the kind suggested by Prof. Elihu Thomson in his paper read before the Institute last spring. I have always regarded Professor Thomson's motor with a great deal of admiration. It would seem from some of the discussions we have listened to, that whatever work we are going to get out of an alternating system, must be a question of difference between two factors opposed to each other—the self-induction of the circuit and the power generated as well as the heating. Inasmuch as all our results must be due to the difference between the two quantities, I have worked upon that line, trying to give that difference its greatest possible chance to do some work. If you will remember, Professor Thomson's motor, as Dr. Duncan has explained it to-night, consists in a fixed field and a revolving armature, the circuit of which was closed when it came into position of action; at that point the circuit is momentarily closed.

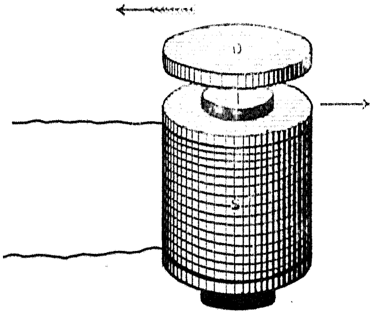

Figure 9.

Fig. 9 illustrates this principle. *S* is a solenoid with a soft iron core *I*, and *D* is a closed circuit of high self-inductive capacity, preferably a copper disc. When alternating currents are sent through the coils of the solenoid *S*, and the centre of the disc *D* is not immediately in the prolongation of the axis of the solenoid, there is a tendency to separate, as indicated by the two arrows. As in all forms of alternate current dynamo-electric machinery, the effective effort is always due to a difference between two unequal and opposing actions, so in any machine in which the principle thus described finds application, the rotary torque or moment of rotation will be due only to a difference of two efforts, as shown in the preceding analysis; and the machine here illustrated is a simple mechanical arrangement of parts, the purpose of which is to cause this tangential effort, however small, to work at a possible maximum of advantage. Those who recall the forms of machine illustrated in a former lecture and based upon this principle, will remember a form of machine not differing widely from the ordinary direct current machine. The armature placed at the centre of the machine consisted of a number of coils of high self-inductive capacity, the different circuits of which were closed in succession as they arrived in the position of maximum repulsive action. In the machine I have designed the application of the same principle is reversed with the apparent effect of producing a more continuous rotary effort, as well as a greatly increased moment of rotation by virtue of placing the point at which the effort is applied farther from the axis of rotation, and thereby giving to the force at work, however small, a much greater lever-arm. Figs. 10 and 11 are end and side elevations of the machine, and Fig. 12 is a diagram of circuits and connections. A polygonal frame *F F F* is connected by lateral strips *N N*, and all is supported upon the base *B B*. Secured to the lateral stripes *N N* are two sets of copper discs *D D* and *d d*. These discs constitute the armature of the machine; they are arranged peripherally, as shown, fixed to the frame structure, and consist of circuits of high self-inductive capacity that remain perfectly closed.

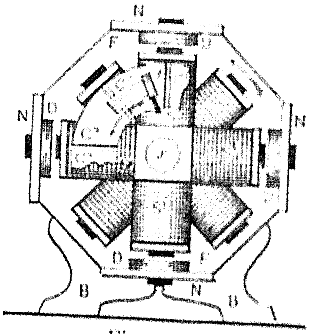

Figure 10

A spindle *x x*, Figs. 10 and 11, carries a revolving switch

C^1 C^2, or sunflower which is secured to the spindle and turns with it. This sunflower commutator constitutes one terminal of the machine, while an insulated ring *b* +, and its contact brush secured to the support *P* *P*, constitutes the other. Under each set of discs, *D* *D* and *d* *d*, and secured to the spindle *x*, is a set of solenoids. Those of one set being placed at an angle of 45 degrees to those of the other set, from which it results that these two sets of solenoids will each come alternately into action with respect to its own set of copper discs. Each set of solenoids has its four coils connected in series and the two sets are arranged in two independent circuits. Both have one terminal secured to the contact *b* +, and the other terminal to the alternate segments of the revolving sunflower. This system of connections is shown in Fig. 12, in which the eight-part commutator is shown with alternate segments connected to each other in separate series; the

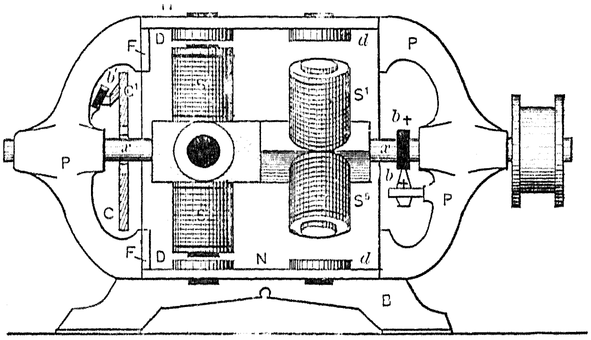

Figure 11.

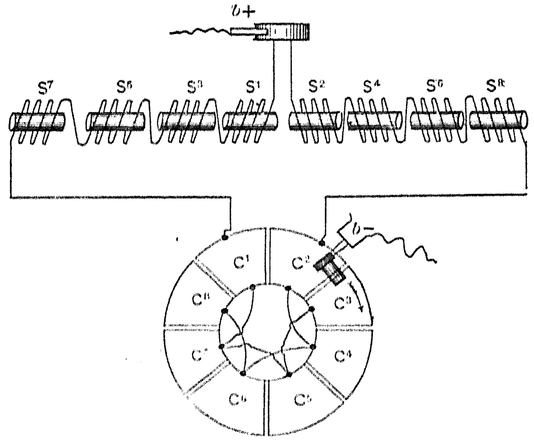

Figure 12.

coils S^1, S^3, S^5, S^7, form one set of revolving solenoids connected in series from the rubbing contract $b +$ which forms one terminal of the machine, to the commutator segment C^1, and thence to all the odd numbered segments. In like manner the other set of solenoids S^2, S^4, S^6, S^8, are similarly connected in series to all the even numbered segments, C^2, C^4, C^6, C^8. From this arrangement it results that as the spindle revolves, carrying these solenoids as a sort of fly wheel, the alternating current will be sent in rapid succession first through one set of solenoids, and then through the other, and by suitably placing this commutator upon the spindle it can be made to send the current through the different sets of solenoids during the periods of maximum effort of repulsion between each set and its corresponding set of copper discs.

The moment of rotation is evidently a maximum under this system of construction, and by increasing the number of sets of solenoids that follow each other in action, the effort of the rotary torque may be made nearly continuous and constant.

It is evidently not contemplated that this motor shall drive horse cars. Its efficiency is not high, and its form renders such an application quite impossible, but it can find a place in the smaller industries, where light motors fill a definite requirement, and render an alternating current circuit a paying one during the working hours of the day.

MR. R. M. HUNTER: The question is one which is very familiar to me practically. I am at present building a motor of eight horse power for our work in Philadelphia which I think will overcome a great many of the difficulties that have been suggested here this evening. We have had tried a great many experiments, in motors having field magnets in which the cores were laminated and also solid. We used laminated cores and we have abandoned them for solid cores in continuous current motors. We now use solid forgings for the field cores. The laminated cores are slightly more expensive, that is if properly constructed; but they have not the great objection that has been urged against them here this evening, if they are constructed properly. That is to say you can adopt a mechanical construction for the cores of these field magnets which is economical in an alternating current motor. In our connections of motors with a car, we can easily get three-fourths of a revolution of the armature shaft before the car is fully in motion. Therefore we can get a start with the motor before the full load is thrown upon it. Of course, in practice, the car gradually starts; but at the same time we do get that effect, for the relative difference in speed between the speed of the motor shaft and axle at starting is far greater than after the car is fully in motion. We start our motor and get a certain portion of a revolution without a particle of movement in the car. I certainly expect to see an alternating current motor used on electrical cars. The question of alternating motors with secondary generators is one that has been familiar to me for a good many years, and one which I have looked forward to as a possible future in electrical railroading, particularly on account of the great objection to large conductors where we are using line conductors; that is where we do not propose to use the secondary battery and where we have a large number of cars on the same circuit.

MR. WARING:—I fail to perceive the particular advantage in running street cars of applying alternating currents for the transmission of power. The principal advantage of the alternating system is lost when the E. M. F. has to be reduced, and in the present state of the art of electric transmission of power it is not practicable to transmit power—even continuous currents—at any higher electromotive force than probably seven hundred volts, especially in street railways, with bare over-head conductors. Therefore I would like to say that I cannot see any particular advantage in using an alternating current when a continuous current will do.

MR. WHEELER:—As Dr. Duncan's explanations are so clear, I think it would be interesting to have him continue, and I would like to have him tell us something about the comparison of the torque of the alternating current motor with the direct constant current and the direct constant potential motors, if he is willing.

DR. DUNCAN:—With regard to using alternating current motors for street cars, I cannot see the advisibility of using them at present, until we know a good deal more about them, while we have continuous current motors which I think very much better adapted to the work.

With respect to the torque of the alternating motor; it will be a maximum when the period of the electromotive force is the same as the period of the counter-electromotive force. For instance, if you have two curves, that (illustrating) representing the curve of the main line or the curve of the dynamo which is supplying the car, we cannot get a greater maximum product than by using a curve whose period is the same and whose phase is not very different. If we had a number of reversals of the impressed

E. M. F. that way, (illustrating) of course, the product would be very much less. The greatest torque will be then when the reversals of the counter-electromotive force are the same as the reversals of the current; that is when the motor is at its normal speed. Of course, for street car work we want to start at a very slow speed, and with an ordinary type of alternating motor—I do not refer to any special types which have been brought forward and more or less described—but with an ordinary type of alternating motor the torque at first is very small indeed; and again, if we are to feed our motor from a determinate source of power, I do not see myself how we can make the torque great at a slow speed. Of course, if we make the current an alternating current by commutating after it gets on the car, then the period of our alternation of electromotive force and counter-electromotive force, can be made the same, no matter what speed we use. But if we are to drive that motor from a dynamo that has a rapid number of reversals, I cannot see that the torque will be great, unless the dynamo has in reality reached its maximum speed. For the maximum work which can be obtained from an alternating motor, Mr. Hutchinson has kindly worked out the problem and finds that if we make the ordinary suppositions and work the motor at its maximum, the expression for maximum work is a very simple one, $\frac{E^2}{8R}$, where E is the maximum value of the electromotive force and R is the resistance of the machine. It is independent of the self-induction or of any other data, providing we build the machine so that it will give the maximum work. There are conditions which Mr. Hutchinson finds in his equations which will enable us to design a machine to give this maximum work. After we have designed it, the maximum work to be obtained is $\frac{E^2}{8R}$. That is about equal to the work that would be obtained from a corresponding source by a continuous current motor.

There is another very curious thing: in order to get the maximum work from a motor—taking some results which we have obtained from a Siemens alternating dynamo—the counter-electromotive force should be greater for this particular case than the electromotive force of the dynamo. If the electromotive force of the dynamo is one thousand volts, we find that the counter-electromotive force should be almost fifteen hundred volts. The torque, as I said, would be greatest when the machine is running at its full number of revolutions.

Mr. Crocker speaks of an alternating current motor, when a load is suddenly thrown on, gradually bringing the load up to its maximum speed, and the belt squeaking. If he tries the experiment he will not find that the belt squeaks, but the motor will suddenly stop, for unless the load is brought up to its full number of revolutions in one period of reversal--in the Westinghouse system, in $^1/_{300}$ of a second—the motor will stop. Of course some arrangement has to be advised to prevent this. Mr. Crocker speaks too, of only using these motors where the maximum load will not be thrown on suddenly. Of course that is limiting their use in such a way that the motors could hardly be commercially applied.

I cannot see that an alternating motor such as I have described, that is a dynamo working backwards with a continuous current to start it and excite the field, will not be at once practical. You might say that the plan complicates the motor by putting an additional commutator upon it. But the commutator is of the simplest kind. There is no wear of the brushes except in starting it, because after the machine is started the brushes are raised; after it has reached its full number of revolutions, you have no wear of the commutator or sparking at all.

Lieutenant Patten has brought forward a very interesting form of machine, a modification of Professor Thomson's plan, which we cannot but regard with the greatest admiration. The difficulty as I see it now, in such a motor is that, as I understand it, Lieutenant Patten has revolving electro magnets that come opposite copper pieces. Am I right?

LIEUT. PATTEN:—That is the idea.

DR. DUNCAN:—The only difficulty in this is that currents must be induced in those copper pieces in order to cause the revolution of the armature. Now what becomes of the energy of those currents? Evidently it is lost in heat. If the energy of rotation of the motors is very great, the current induced in those pieces of copper is great. Therefore the loss must be great. For myself, I think that the two probable forms of motor are the motor I have described and some modification of Professor Thomson's motor. Whether Lieut. Patten has arrived at that modification I do not know, and, of course, cannot tell without further data and experiments.

Mr. Crocker again speaks of the sparking of the alternatin motor being a minimum when it is regulated. I do not believ in alternating motors that have commutators, that is, that have number of bars through which the current is constantly flowing I have tried experiments with continuous current dynamos worke as motors, and find very little effect from them. In order to get a large output we must have the counter-electro motive force of the motor equal to its self-induction, and that is simply impossible in practice. If you join up an ordinary series motor with a source of alternating electromotive force, and then with a corresponding source of continuous electromotive force; in the first case you can stop the motor by just laying your hand on it; with the continuous current you have a comparatively immense power.

With regard to laminating the cores of fields I am very glad to hear that it is a non-expensive process. I have no doubt, if such is the case, it would have a distinct advantage in alternating dynamos of every class, because the alternations of current in the armature must cause induced currents in the poles, and if we can build a laminated pole more easily than we can a forged pole, I think it would be policy to make all our poles laminated. However, I have had no experience at all on this matter and can hardly discuss it.

Mr. Waring made some remarks which embody my sentiments with regard to using alternating current motors for street cars. I do not see the use of an alternating current motor on a street car. The only difficulty in a continuous current motor lies in the commutator and the brushes. There is trouble, I suppose when the car has to go both ways. Now, that is a matter of detail, and with properly constructed brushes there should be very little trouble. If the commutator bars were made hard enough we would have very little wear; if they were made properly we have absolutely no sparking. I think I saw somewhere a reference made by Dr. Hopkinson to a method of preventing sparking. It is well known in laboratory work. There is a brush bridge over the coil. The current goes through the short circuited coil, and through the brush out to the main circuit. Now when the armature reaches a certain position, the short circuited coil is suddenly thrown into the main circuit. The effect is to suddenly change its current, if its current when short circuited is not exactly the same as half the current in the main line. Now a change of current taking place in an infinitely short time, would develop an infinitely high electromotive force. It does not take place in that time, but rectifies itself by an arc, the current being still partially short circuited. This effect can be obviated by having behind the main brush another brush; the resistance between them being four or five ohms, or something of the kind, as shown in Fig. 13. Then the current will rectify itself by passing through that auxilliary brush, and though some energy will be lost, still not more than in sparking, and there will be no wear of the commutator.

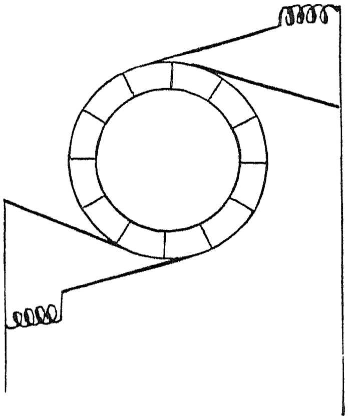

Figure 13.

I think I have answered all the questions that have been asked; and I will say again that I have a firm faith in the ultimate success of alternating current motors, and I think that the two forms to which we must look for results are either simply the dynamo reverse, such as I have suggested, or some modification of Professor Thomson's dynamo. [Applause.]

THE PRESIDENT:—I think it must be a source of great gratification to all of us, as members of the Institute, to see from the records of the society how far the papers which have been bro before us during the past year have served as the basis of very interesting and important investigations into the subjec alternating currents. The paper which we have had bef this evening has been a very valuable contribution to th ture of the subject, and it has afforded us all very great and profit to listen to it.

Adjourned.

A paper read before the American Institute of Electrical Engineers, April 10th, 1888.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

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THE MAXIMUM EFFICIENCY OF INCANDESCENT LAMPS.

BY JOHN W. HOWELL.

The word efficiency, when applied to an incandescent lamp, is used to designate the amount of energy required by the lamp for the production of a given amount of light; thus we say that a given lamp has an efficiency of three watts per candle, at sixteen candles, meaning, that to produce an illumination of sixteen candles we must supply the lamp with forty-eight watts.

The word efficiency, when applied to a prime mover or to any piece of apparatus that changes energy from one form to another, or which transmits or utilizes energy, has a well-defined meaning, and is used to represent the ratio of the energy of the useful effect produced by the apparatus, to the energy necessarily supplied to the apparatus to enable it to produce that effect.

An incandescent lamp transforms electrical energy into heat and light, so the use of the word efficiency to denote the watts per candle required by the lamp is not a proper one. To denote properly the efficiency of an incandescent lamp we must be able to separate the energy of the light produced by it from the energy of the heat produced. Then the ratio of the energy of the light to the electrical energy required by the lamp will be a correct expression for the efficiency of the lamp, and we will have to find some other word to designate the watts per candle. In this paper the word efficiency is used in its ordinary improper sense to denote the watts per candle required by a lamp when producing a given amount of light.

The efficiency of a lamp varies with its candle power. The curve, Fig. 1, shows the rate of this variation for a particular lamp. At 5 candles this lamp has an efficiency of 6.7 watts per candle, at 10 candles it is 4.2 watts per candle, and at 20 canc it is 2.66 watts per candle.

Any statement regarding the efficiency of a lamp must the fore, be accompanied by a statement of the candle power at wh it has the stated efficiency; without this it is meaningless. Th is nothing in an incandescent lamp itself that fixes its pro

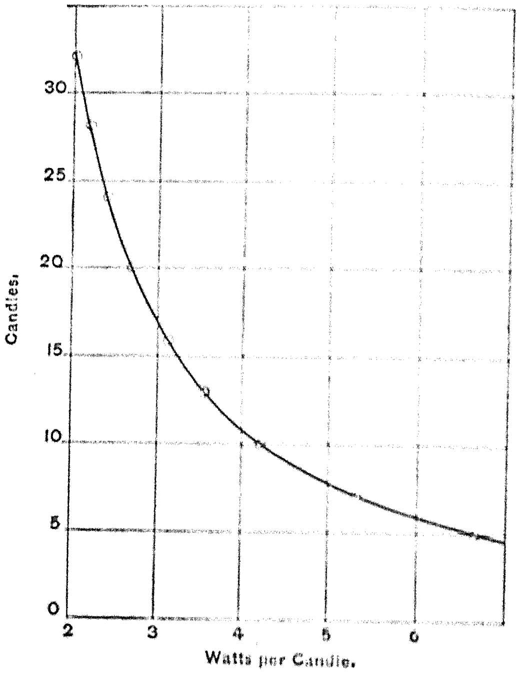


Fig. 1.

efficiency or in any way indicates what it is. The lamp fro which the curve, Fig. 1, was determined has, within the limits the curve, any efficiency between 2 and 7 watts per candle. Thu by simply changing the candle power of the lamp, we can operat it at any efficiency we choose, and get as much or as little lig per watt as we choose.

In commercial practice the candle power of lamps is alway marked on them and their efficiency at this candle power i stated; but even this is not a proper index to the value of th lamp or to its proper efficiency. Experience has shown tha lamps are almost universally run above their normal rating lamps rated at 4.5 watts per candle are usually run at from 3. to 4 watts per candle, and in order to make lamps that will stan the strain of being run above their rated capacity, it is necessary to rate them considerably below the efficiency at which they will give the best results under ordinary circumstances.

Lamps have been made and sold in England which have a very high rated efficiency, but parties buying these lamps are told that they will get very much more satisfactory results if they run the lamp *below their rated capacity.* So we see that some lamps are rated above their capacity and some are rated below, and the rated efficiency of a lamp is not always the best efficiency at which to run it. How then, are we to determine the efficiency at which a lamp will give the best results? It is this question which I will attempt to answer.

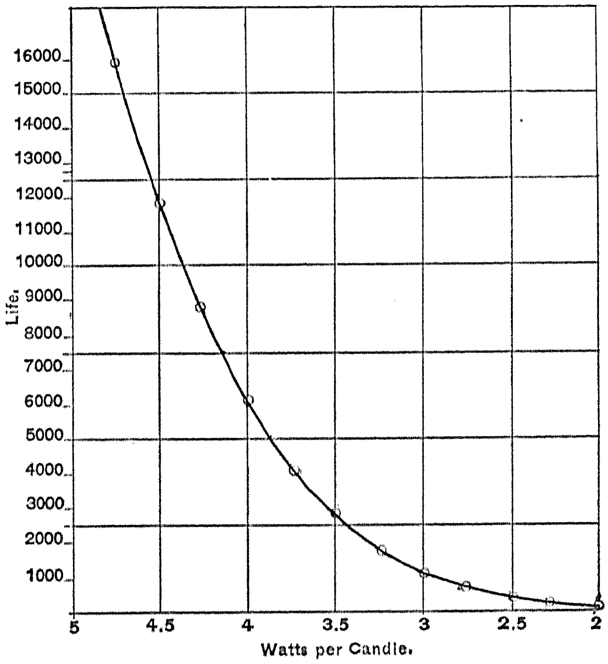
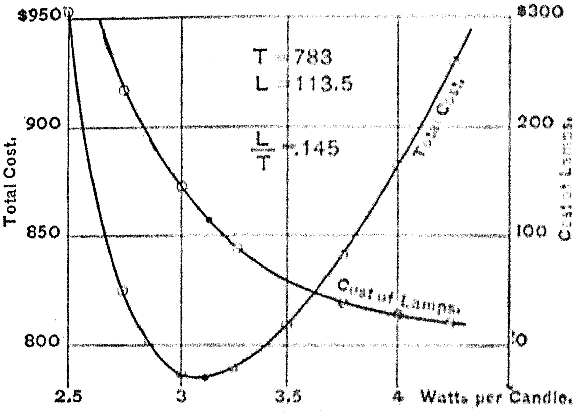

Curve showing lives of equally good lamps, burned at different efficiencies.

Fig. 2.

The term "maximum efficiency" of a lamp, as used in the title of this paper, does not mean the highest efficiency at which a lamp can be operated, but the efficiency at which the best results are produced by the lamp: or more accurately, *the efficiency at which the cost of operating the lamp is a minimum.*

Taken in this latter sense, the maximum efficiency of a lamp is not its highest efficiency. As we increase the candle power of a lamp its efficiency increases; consequently, by running the lamp high enough we can make its efficiency so high that very little power is required to produce a given amount of light, and the cost of power to produce the light is very small. But, while the efficiency of the lamp increases, its life decreases, and if we run a lamp at too high an efficiency the saving in the cost of power is more than balanced by the increased cost of lamp renewals.

Lamps 85 cts. each—Life 600 hrs. at 3 Watts per Candle.
Current costs 10 cts. per HP. per hour.



Minimum at 3.1 Watts per Candle.
∴ Maximum Efficiency 3.1 Watts per Candle.

Fig. 3.

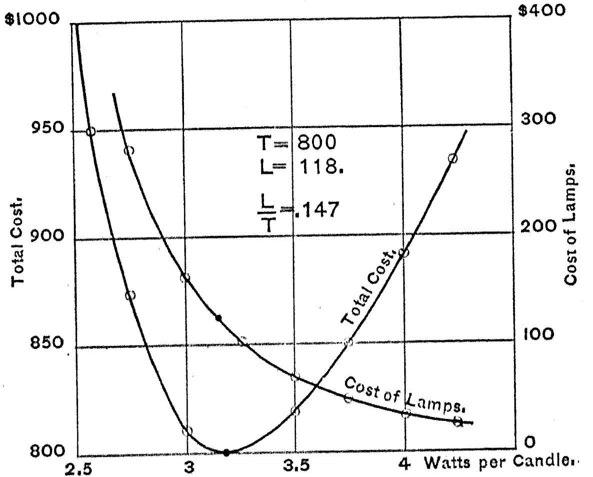
To determine the maximum efficiency for lamps under given conditions, we must determine the efficiency at which the sum of the costs of power and lamps is a minimum, and in order to do this we must know the rate of variation of the life of a lamp with its efficiency.

The curve, Fig. 2, shows this rate of variation. This curve is the result of very carefully conducted experiments made by the Edison company. These experiments extended over five years and consumed a very large number of lamps. Its accuracy when applied to Edison lamps is beyond question; but our experiments with lamps having an artificial surface on the carbon, or flashed" lamps as they are called, show that their rate of tion of life and efficiency follows a different curve.

's *curve does not apply to individual lamps.* If we take two Edison lamps and burn them at different efficiencies, their lives for these efficiencies will probably not be such as indicated by the curve, nor will they be proportioned to these indicated lives. But if we take one hundred lamps and burn them at one efficiency, and another hundred equally good lamps and burn them at another efficiency, the average lives of the two sets will be proportional to the lives indicated by the curve for these two efficiencies.

In order to determine at what efficiency the cost of operating lamps of a given quality under given conditions is a minimum we must calculate what this cost is at different efficiencies. To do this we consider the total cost of operating the lamps to be made up of two parts, viz., the cost of the *current* and the cost of the *lamps*. The cost of the current is made up of every expense incurred in operating the lamps, including materials consumed, labor, taxes, insurance, rent and every other expense incurred in operating the plant, except the cost of lamps. The cost of the lamps is an item by itself, and is the amount which the lamp has cost when it is put in use. This is a natural division of the total cost of operating a plant, since to produce light by incandescence all that is necessary is a lamp and current to operate it.

Lamps $1.00 each—Life 600 hrs. at 3 Watts per Candle.
Current costs 10 cts. per HP. per hour.

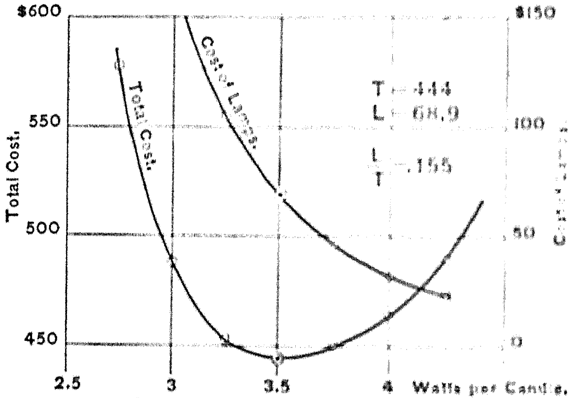


Minimum at 3.18 Watts per Candle.
∴ Maximum Efficiency 3.18 Watts per Candle.

Fig. 4.

If, in any case, we know the cost of the current required to operate the lamps, the cost of the lamps, the quality of the lamps—that is, the life they will give when burned at a given efficiency—and the rate of variation of their life with efficiency, we can then calculate at what efficiency the cost of operating the lamps is a minimum, and this I call the maximum efficiency of those lamps.

Lamps $1.00 each—Life 600 hrs. at 3 Watts per Candle.
Current costs 5 cts. per HP. per hour.



Minimum at 3.5 Watts per Candle.
•˙• Maximum Efficiency 3.5 Watts per Candle.

Fig. 5.

The following examples show what this maximum efficiency is under varying conditions of the cost of lamps, the cost of current and the quality of the lamps. The cost of the lamps I have varied between 25 cents and $1.00 each. The cost of current varies between 2.5 cents and 10 cents per h. p. per hour. The quality of the lamps varies between 300 hours life at 3 watts per candle, and 2,400 hours life at 3 watts per candle.

In each of the following cases I have calculated the cost of operating 100 16 c. p. lamps 1,000 hours, at each of the efficiencies comprised in the curve of total cost. These curves do not show the cost of running the same lamps at different efficiencies, but the cost of running equally good 16 candle lamps of the different efficiencies.

The first case we will consider is shown in the diagram Fig. 3. In this case the lamps are assumed to cost 85 cents each and to have a life of 600 hours at 3 watts per candle. The current is assumed to cost 10 cents per h. p. hour.

The cost of the current is determined from the following formula:

$$\text{Current cost} = \frac{\text{w. p. c.} \times 16 \times 100 \times 1{,}000 \times \text{cost of ct. per h. p. per hour.}}{746}$$

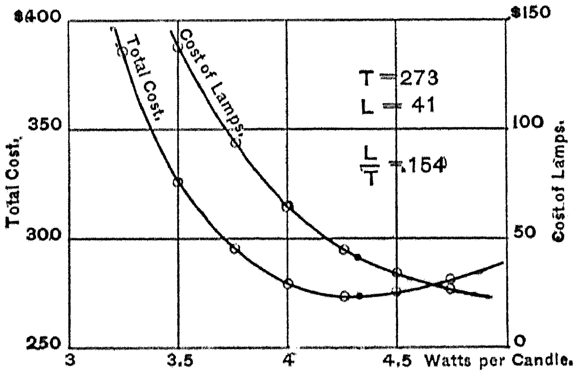
And the cost of lamps from this formula:

$$\text{Cost of lamps} = \frac{\text{cost of one lamp} \times 100 \times 1{,}000}{\text{Life at given efficiency.}}$$

The curve marked *total cost* shows the total cost of running 100 16 c. p. lamps 1,000 hours, the efficiencies of the lamps varying between 2.5 and 4.25 watts per candle. The efficiencies are shown by the vertical lines, referring to the scale at bottom. The value of the total cost at any point of the curve is shown by the horizontal line through the point, referring to the scale at the left of the diagram.

The lowest point of the curve shows the point where the total cost is lowest. This is the minimum cost of operating these lamps under the given conditions. The mark at the lowest point of the curve shows this minimum cost to be $783, and a vertical line through this point to the scale at bottom of the diagram shows that this total cost is a minimum when lamps having an efficiency of 3.1 watts per candle are used.

Lamps $1.00 each—Life 300 hrs. at 3 Watts per Candle.
Current costs 2.5 cts. per HP. per hour.



Minimum at 4.32 Watts per Candle.
.•. Maximum Efficiency 4.32 Watts per Candle.

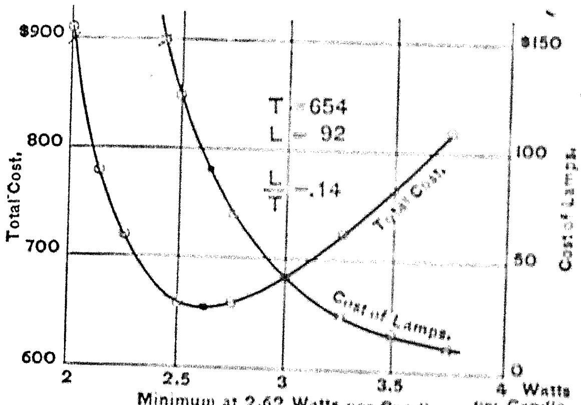
Fig. 6.

Thus the maximum efficiency of these lamps under the conditions assumed is 3.1 watts per candle.

The lamps considered in the case shown in Fig. 4 cost $1.00; all other conditions are the same as in the case shown in Fig. 3. This increases the total cost from $783 to $800, and necessitates using lamps of 3.18 watts per candle instead of 3.1, to make the cost of operating a minimum.

In the case shown in the diagram, Fig. 5, the current costs 5 cents per h. p. per hour; the other conditions are the same as in the case assumed in Fig. 4. The minimum total cost in this case is $444, and to make this cost a minimum we must use lamps having an efficiency of 3.5 watts per candle.

Lamps 50 cts. each—Life 1200 hrs. at 3 Watts per Candle.
Current costs 10 cts. per HP. per hour.



Minimum at 2.62 Watts per Candle.
●● Maximum Efficiency 2.62 Watts per Candle.

Fig. 7.

In this case shown in Fig. 6 the lamps cost the same as in the last, but are only half as good; the current costs just half as much as in the last case. The minimum total cost in this case is $273, and the maximum efficiency of the lamps is 4.32 watts per candle.

In the case shown in Fig. 7 the lamps cost 50 cents and have a life of 1,200 hours at 3 watts per candle. The current costs 10 cents per h. p. per hour. This is the cheapest and also the best lamp we have yet considered, but the current is expensive. In this case the minimum total cost is $654, and the maximum efficiency of the lamp is 2.62 watts per candle. In this case, Fig. 8, the current costs half as much as in the previous case, other conditions being the same. The minimum total cost is reduced from $654 to $362. The maximum efficiency in this case is 2.88 watts per candle. In Fig. 9 the current costs twice as much as in the previous case and the lamps are only half as good. The minimum total cost is doubled, but the maximum efficiency is the same as in the previous case.

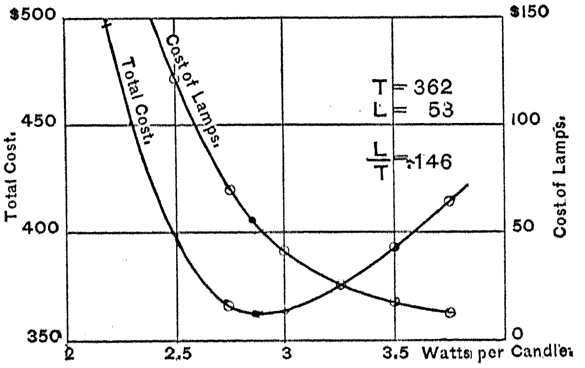
In this case, Fig. 10, the current costs one-half of that assumed in the previous case, other conditions being the same. The minimum total cost is $400, and the maximum efficiency 3.175 watts per candle.

The curve, Fig. 11, illustrates the case of very cheap and very good lamps, with moderate cost of current. The minimum total cost is low, $294, while the lamps are run at the high efficiency of 2.38 watts per candle.

In the case shown in Fig. 12, the cost and quality of the lamps are the same as in the previous case, but the current costs twice as much. This increases the minimum total cost from $294 to $535, and raises the maximum efficiency to 2.14 watts per candle.

In the case, Fig. 13, the cost of lamps and the cost of current are the same as in the case shown in Fig. 11, but the lamps are only half as good. The minimum total cost is increased from $294 to $327, and the maximum efficiency is reduced from 2.38 to 2.62 watts per candle.

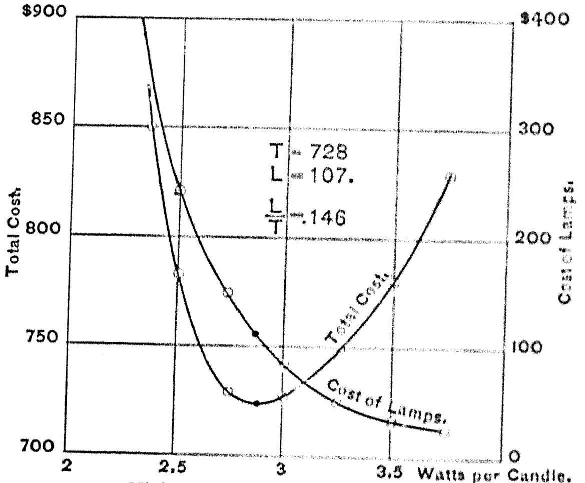
Lamps 50 cts. each—Life 1200 hrs. at 3 Watts per Candle.
Current costs 5 cts. per HP. per hour.



Minimum at 2.875 Watts per Candle.
•Maximum Efficiency 2.88 Watts per Candle.

Fig. 8.

The first and plainest inference drawn from these curves is that the maximum efficiency of any given lamp is not a fixed one, but varies with conditions outside the lamp itself. Identical lamps operated under different conditions of cost of current must be burned at different efficiencies to make the cost of operation a minimum for the production of a given amount of light. In order to determine the maximum efficiency of lamps, therefore, we must know the quality of the lamps referred to some standard, the cost of the lamps to the consumer, and the cost of current under the actual conditions existing at the place where the lamps are to be used.


Lamps 50 cts. each—Life 600 hrs. at 3 Watts per Candle.
Current costs 10 cts. per HP. per hour.

Minimum at 2.88 Watts per Candle.
● Maximum Efficiency 2.88 Watts per Candle.

Fig. 9.

In the eleven cases shown in this paper the lamp of highest efficiency is obtained in the case shown in Fig. 12. This is a case where the lamps are very cheap and very good, and current is very expensive. The lamp having the lowest efficiency is obtained in the case shown in Fig. 6, in which the lamps are poor and high-priced and the current is very cheap.

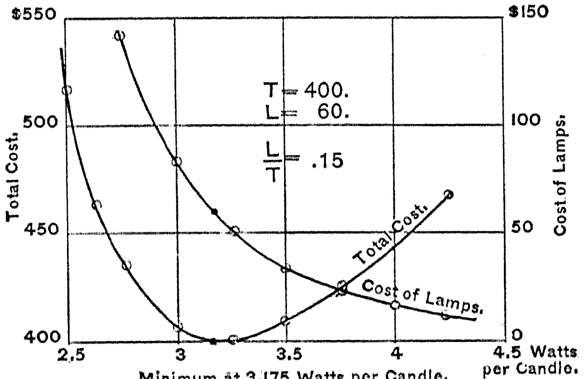
There is a marked difference in the sharpness of these curves at the minimum points. An inspection will show that the sharpness of the bend in these curves depends upon the cost of the current, the curves in which the current costs 10 cents per h. p. per hour being the sharpest. Those, in which the current costs 5 cents, are next, and the one, Fig. 6, in which the current costs only 2½ cents per h. p. per hour, is very flat at the bottom or minimum point. In this comparison Fig. 7 is not considered, as it is drawn on a different scale from the others, and is not as sharp as it should be.

This indicates that the more expensive the power is, the more carefully must the lamp efficiencies be chosen. In Fig. 12, which shows the sharpest curve, a very slight variation in the efficiencies of the lamps makes a very great change in the total cost of operation.

In the case shown in Fig. 6, in which the current is very cheap, we find that a very considerable change in the efficiency of the lamps used makes very little difference in the total cost of operation.

On each of the eleven diagrams two curves are drawn, one showing the total cost of operation, and the other showing the cost of lamps. On each of the curves showing the cost of lamps, a point is marked which indicates the cost of the lamps when the total cost is a minimum. The letter T marked on each of the figures denotes the minimum total cost of operating the lamps under the given conditions. The letter L denotes the cost of the lamps when the total cost is a minimum; and the expression $\frac{L}{T}$ denotes the ratio of the cost of lamps when the total cost is a minimum, to the minimum total cost. An examination of all these curves shows that while the minimum total cost varies with each of the three quantities—price of lamps, quality of lamps, and cost of current—nevertheless, *the total cost is always a minimum when the cost of lamps is about* 14.5 *or* 15 *per cent. of the total cost of operation.*

Lamps 50 cts. each—Life 600 hrs. at 3 Watts per Candle.
Current costs 5 cts. per HP. per hour.



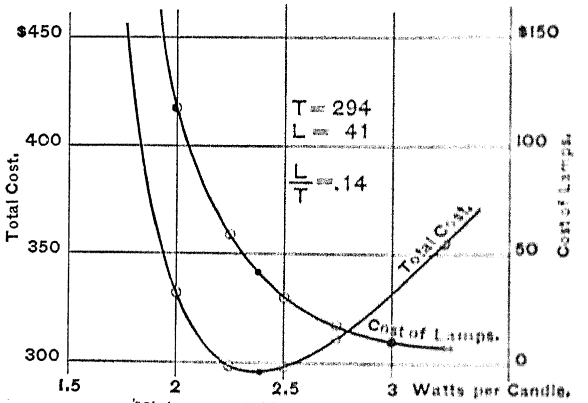
Minimum at 3.175 Watts per Candle.
∴ Maximum Efficiency 3.175 Watts per Candle.

Fig. 10.

This figure varies somewhat in the different examples considered, but the variation seems to follow no law. In Figs. 6 and 12, which show the highest and lowest efficiency lamps, the figures are 15.1 per cent. and 15.4 per cent. respectively.

The steepness of the curve showing the cost of lamps, and the difficulty of determining the exact minimum point of a curve which has been drawn by inaccurate methods, makes it difficult to get the cost of lamps accurately when total cost is a minimum. These curves and values are given just as they were determined, and no effort has been made to bring the results into closer agreement, as could readily have been done. I consider the variation shown by these curves to indicate closely enough that this ratio of cost of lamps to total cost at the minimum point is nearly, if not quite, constant and that its value is between .145 and .15.

Lamps 25 cts. each–Life 2400 hrs. at 3 Watts per Candle.
Current costs 5 cts. per HP. per hour.

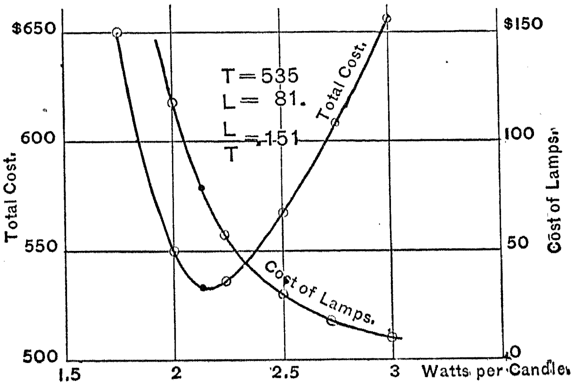


Minimum at 2.38 Watts per Candle.
●●●Maximum Efficiency 2.38 Watts per Candle.

Fig. 11.

This establishes a very simple law for determining whether or not lamps are being operated at their maximum efficiency; for if they are, the lamp bills will be about 15 per cent. of the total operating expenses of the plant. If the lamp bills are more than 15 per cent. of the total operating expenses the lamps are being burned above their maximum efficiency, and lower efficiency lamps should be obtained. If, on the other hand, the lamp bills are less than 15 per cent. of the total expenses, the lamps are being burned below their maximum efficiency, and higher efficiency lamps would reduce the cost of operating the plant. Where fuel is high priced, or where other causes operate to make the cost of generating current high, it is specially important to use lamps of the maximum efficiency, for we have seen from the above curves that where the cost of current is high, the use of lamps whose efficiency is a little above or below the maximum efficiency attainable under the conditions of operation, makes a very marked increase in the operating expenses of the plant.

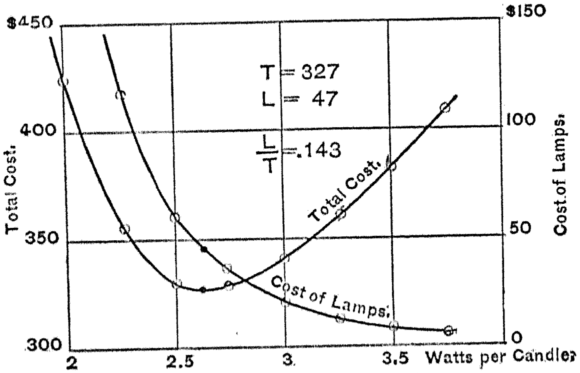
Lamps 25 cts. each—Life 2400 hrs. at 3 Watts per Candle.
Current costs 10 cts. per H.P. per hour.



Minimum at 2.14 Watts per Candle.
∴ Maximum Efficiency 2.14 Watts per Candle.

Fig. 12.

Lamps 25 cts. each—Life 1200 hrs. at 3 Watts per Candle.
Current costs 5 cts. per H.P. per hour.



Minimum at 2.62 Watts per Candle.
∴ Maximum Efficiency 2.62 Watts per Candle.

Fig. 13.

If in any plant the lamp bills are only 10 per cent. of the total expenses, then *increasing the efficiency of the lamps by increasing their candle power does not diminish the total cost of* *operating the plant.* In order to reduce the total cost, *the lamps must be replaced by lamps of the same candle power but of higher efficiency.* If the efficiency of the lamps is increased by raising their candle power, the cost of operating the plant *per unit of light produced* is reduced, but the total cost is increased. A plant which is paying less than 15 per cent. of its total expenses for lamps, and which brings the lamp bills up to 15 per cent. by increasing the candle power of the lamps, does not decrease the cost per lamp of operating the plant, but *does* decrease the cost per candle of light furnished. If they are paid for the increased light given by the lamps, the efficiency of the plant is made a maximum for the existing conditions; but if they are not paid for the additional light furnished, the efficiency of the plant is reduced.

This law enables any one operating an incandescent lamp plant to determine whether or not he is using the most suitable lamps for his plant. If the conditions of operation of a new plant are all known, and the quality of the lamps made by any lamp maker is known, we can determine before starting what is the most economical lamp to use.

DISCUSSION.

MR. S. S. WHEELER:—I am very glad that Mr. Howell has given us the results of these experiments without any attempt to alter them, because I think that when slight variations are seen in experiments we have greater faith in their accuracy. I understand that the curve of minimum total cost is the mathematical sum of two other curves, the curve of cost of the lamps and the curve of cost of power. The curve of cost of lamp is there on the chart, and I would like very much to see the curve of cost of electricity. As I understand it, it is a straight line.

MR. HOWELL:—Not at all. If you want to see it, you can plot it from these values.

MR. TOWNSEND WOLCOTT:—As I understand from this diagram the ordinates for cost of lamps are not the same as for the total cost.

MR. HOWELL:—No.

MR. WOLCOTT:—So strictly speaking that ought to be entirely below.

MR. HOWELL:—Yes, it ought to be. But to do that we would have to bring the paper away down below, because that cost of lamps comes very nearly down to zero.

MR. WHEELER:—I think it would be an interesting thing to have one of your charts—one of the characteristic ones, with the curve of cost of electricity on it—so as to complete the analysis of the cost. It ought to be a straight line.

MR. HOWELL:—That could be very easily made. (The speaker plotted the curve of electricity on the chart.) It *is* a straight line.

MR. JOSEPH WETZLER:—I would like to ask Mr. Howell if he could base calculations of both central stations and private plants on these diagrams and results, and if so, what the method of differentiation would be for separating the income or expenses of a station which at the same time supplied both power and light—if there be any method of determining that.

MR. HOWELL:—Mathematics does not know the difference between a central station and an isolated plant. Of course, there is no difference. In regard to differentiation, that is a matter of book-keeping entirely. A man has got to know the conditions of his plant in order to tell how much of his power he has got to charge against his motor running and how much against his light. Of course that cost of current includes every expense incurred in operating plant, except the price of lamps. I have got to treat the matter in that way, because what I am after is to get the ratio of cost of lamps to total cost; so I take the cost of lamps by itself, and leave everything else with the cost of current. In an incandescent lamp all you want is a lamp and current and you have got your light. If you know how much your lamps cost and how much your current costs, you have got your total expense. I have charged everything against cost of current, because every expense is incurred in either furnishing lamps or furnishing current to run them. A lamp is a known and definite thing, and everything else I have charged against current. In operating a motor, if you get the cost of current per horse power per hour, all you have got to do is to charge against the lamps the horse power consumed by them, and against the motors the horse power consumed by them, and if you know the amount of electricity used in your motors and in your lamps, the proper proportion of that will be the cost of current in each case.

MR. CHARLES WIRT:—It happens sometimes that power costs so much you can increase it very cheaply, but if you figure *pro rata* it gives you a larger cost. You are using one hundred ampères, and to get the cost of an ampère you divide the total cost by one hundred; that gives you ten cents an hour. But you can furnish a few additional amperes at very slight cost.

MR. HOWELL:—Of course in any case like that, your figuring would not amount to much. I do not suppose any calculations of that kind are of much use unless you apply them on a very large scale. Suppose I know from my records of a station for a whole year my total expenses for that year, the differences between that and my lamp bills will show me the total cost of current. I have also got to know, besides the total expenses, the current produced, the average per month, or the average per year—or differentiate it as much as you like. Of course if you applied that to any given time or any short time, you would find one day very different from another, and you would have to change your lamps every day.

MR. F. B. CROCKER:—I would like to ask Mr. Howell if he thinks those maxima points on the chart should be varied slightly in order to have the lamps break less frequently in deference to the question of human nature. Any one would be willing, I think, to pay a little more for his current rather than have to replace his lamps more frequently, or rather than have one break just as he is reading. It is very annoying. I think almost every customer would like to increase the life of his lamps even though he had to pay more for his current. Do you in practice consider such points as these?

MR. HOWELL:—In any such case the only way you can allow for that on a curve, is to get a money value for that man's inconvenience.

MR. CROCKER:—I do not mean that such a question can be reduced to mathematics; I mean, as a matter of fact, do you modify those results?

MR. HOWELL:—As I say you can make an arbitrary curve or get a money value for the man's inconvenience and charge that against the cost of the lamp. Of course that would make the lamp cost more. The lamp costs not only what you pay for it, but what you have got to pay that man to keep him from growling, and of course that would make the lamp cost more. Or, you can guess at it, if you like.

MR. E. T. BIRDSALL:—I think after the lamp had burned the maximum number of hours, the customer would rejoice at the breaking of it.

Mr. C. O. Mailloux:—That suggests another point, which is the fact that the lamps will not remain of normal candle power, but that like all other things in nature they grow old and weary, and they soon reach a point where it is a question whether or not they are darker than the surrounding darkness. I would like to ask Mr. Howell whether the life of the lamp, as it is taken here in these curves, means the time at which they break, or the time at which they become so dark that they no longer give the same candle power, or at least if they are giving the some candle power, it is well known that the watts per candle-power must have increased.

Mr. Howell:—The life in all lamps is, I think, taken on a new lamp. When any man in the business speaks of an incandescent lamp he means a new one.

Mr. Mailloux:—It is not new after it has burned 1,600 hours.

Mr. Howell:—No; you are quite right. But still if you take lamps and consider them when they have burned half their lives, or consider a new one, or consider one two-thirds gone—if you consider them all at the same point you can get a law. You cannot get a law if you consider some new, some old, and some three-quarters old. Of course the easiest point to take, the one of least trouble, is the new lamp.

Mr. Mailloux:—Then you take the point when the lamp is new.

Mr. Howell:—Certainly.

Mr. Mailloux:—And let it burn until it burns out?

Mr. Howell:—Yes, sir.

Mr. Mailloux:—By the time it gets old, it is not giving the same candle-power.

Mr. Howell:—It makes no difference; the law applies just as well. You use the same data on your lamps as you do in determining your law. If you find you want lamps 3.1 c. p. to give maximum efficiency, you take them when new. If you made your curve by taking data of the lamps half old, you would have to do the same thing here. If you start with one point of reckoning, you have to keep it up in all your calculations and experiments.

Mr. F. R. Upton:—A 16 c. p. lamp in practice commercially fills a certain place, and as measured by the makers it can be taken as a commercial unit, and it is to this that the curves are applied. In this curve (Fig. 2), for example, 3.1 watts per candle is the best efficiency for the commercial man. The current costs ten cents; the lamp, eighty-five cents.

Mr. Mailloux:—I do not think the significance of what I said is fully understood. It occured to me on looking at the lamp as one of those things which is constantly varying in its light-giving power, that it would react somewhat on that part of the curve which refers to the cost of lamps, because, certainly a lamp of low efficiency would, other things being equal, be preferable, from the fact that it would have a more uniform candle-power and would last longer, probably, before it became dim, and on the other hand a lamp of very high efficiency would soon become dark and would have to be changed. Now it is quite likely that a lamp of high efficiency would have to be changed long before it had broken, and what I meant was whether the cost was to be taken at the time when the customer had got tired of it and had called for a new lamp or whether it included the growling you would allow him to do before being induced to change the lamp.

Mr. Howell:—In practice you find it universal almost that lamps are taken down before they break. Of course, that affects directly the cost of the lamps, because a central station, furnishing lamps for nothing, gives a man a lamp when it breaks and also when a lamp becomes so black that you have to put a new one in. Of course, a lamp handed to you as being useless, is thrown into the receptacle for broken lamps and is a dead lamp, so it includes lamps only to the point at which they become useless.

Mr. Upton:—The assumption on those tables is six hundred hours. That means, if you get that average light out of the lamp that the curve will hold true. Now the intention of makers of incandescent lamps—I know it is with the Edison Lamp Company—is to make lamps which for six hundred hours will give their full efficiency in candle power. After that there is no guarantee and nothing said about it. We have now a set of new lamps—a hundred of them—which have run four hundred hours, and only four of the lot have broken. It has always been the intention and always the claim that when a lamp is blackened that it should be destroyed. You will notice that the Edison claim has been for a low guarantee, and one reason for that low guarantee is, that the average life of the lamp is not, how long it will stand, but how long it will keep up good commercial efficiency, and, what happens to the lamp after six hundred hours is of no consequence to this. The assumption is six hundred hours at 3.1 watts per candle-power.

I think Mr. Howell did not call attention to Fig. 2. I think great consideration should be given to it. It represents, as he says, five years experience on the part of the Edison lamp people, and I remember very well that in 1879, at Menlo Park, Mr. Edison spoke a great many times of the great value such a curve would have, and also of the exceeding difficulty of getting such a curve; because you have not only to pick out a large number of lamps from the same lot and test them under normal conditions, watching them closely all the time; but to get a good curve, it requires many months and even years of constantly keeping lamps burning, and this represents the full experience of the Edison Lamp Company, which just before Mr. Howell prepared this paper was carefully gone over, taking in all the lamps which had ever been burned, and I think it is really stated safely, to be within a two per cent. error. Shouldn't you say it was within two per cent?

MR. HOWELL:—I have not given that point any thought. It is practically accurate. That curve, Fig. 2, I do not think could be reproduced on any lamp for ten thousand dollars. What do you think, Mr. Upton?

MR. UPTON:—It has cost more than that to get it.

MR. HOWELL:—I don't think it could be done over again for ten thousand dollars.

MR. UPTON:—It is the most expensive curve in the business. It is a great deal harder to obtain than most people give credit for. It runs into a very difficult experiment. First you have to get your higher values. You cannot have them contemporaneous with the others, because the higher values of lamps will break off quickly; the others will hang on for many weeks and months; and to get your proper values throughout that curve, you must know that your regulations and all your conditions are correct. We commenced that relative value curve at Menlo Park, and it is only recently that we felt certain that that, as shown there, expresses the Edison bamboo untreated filament.

MR. WETZLER:—I would like to ask the speaker, if he has evolved the mathematical expression for that law. What formula corresponds to that?

MR. HOWELL:—The law of that curve is a very complex one.

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Mr. Wetzler:—I would like to ask the speaker, if he has evolved the mathematical expression for that law. What formula corresponds to that?

Mr. Howell:—The law of that curve is a very complex one.

It is not a simple law at all. We have got an expression for it, but it is nothing that you can handle mathematically. I have had a mathematician at work on that for about a week lately to see if he could handle it. I have tried it before and could not handle it mathematically.

MR. WETZLER:—It looks like a hyperbola.

MR. HOWELL:—It is not a hyperbola, or any other curve that has a name. We have got an expression for it that goes all the way across a sheet of paper. It is an exponential law with a constant increment, and the formula is very difficult to get. The graphical solution is very much the easiest.

MR. WETZLER:—As a little digression, I would like to ask if the speaker has ever verified the law for the candle power of the lamp with reference to the current passing through it.

MR. HOWELL:—No, Mr. President, I have not, for the simple reason that current passing through a lamp in incandescent work is something that we do not talk about. We talk about the pressure on the lamp always. I have never verified that with reference to currents, although it is a very simple thing to do. I have all the laws of the lamp referring to candle power, efficiency and all that, but I have never touched the current. We hav got all the data but have not worked it out.

MR. WETZLER:—If we assume the resistance of the lamp to remain constant, it evidently is the same function whether we use current or potential.

MR. HOWELL:—We must not assume something that is not so, because the resistance does *not* remain constant. If you were to say that the light of the lamp varied as the sixth power of the current, that would not mean anything to an average user of lamps, because all we know about in practice is pressure, and when you tell a man that the light given by a lamp is the sixth power of the current, it really does not mean anything to him at all, because he does not know how the current varies with pressure. If you tell him the variance between light giving power and pressure, he understands that.

MR. WETZLER:—I merely put it that way because I believe the law was stated at one time in currents, not in potentials.

MR. MAILLOUX:—I would like to ask Mr. Howell if he has any data, or if he knows of observations which have been made tending to show clearly the difference in life of lamps run at the same watts per candle power, using different currents. I suppose that we are all familiar with the statement which went the rounds of the press a year or two ago, to the effect that an alternating current added life to the filament of the lamp; that a lamp run at the same watts per candle-power, at the same efficiency, as Mr. Howell has called it, with an alternating current, would last longer than a lamp run at the same efficiency with a continuous current. If that is true it would certainly be very interesting to have some data by which we could see how great the difference is, because we would naturally expect that the difference must be capable of being differentiated further, and that we would find even different machines capable of showing the difference in the light. Now I may add that my experience with storage batteries has shown that the life of a lamp is undoubtedly increased at the same efficiency—that a lamp running at three watts per candle power, for instance, with storage batteries, will last much longer than a lamp running at the same efficiency from an ordinary current as furnished by a dynamo. How much the difference is I really cannot say; though, as I have said, there seems to be undoubtedly a difference. Now, perhaps Mr. Howell can give us information on that subject which would be very welcome and useful.

Mr. Howell:—To determine the life of any lamp there is one absolute necessity, and that is a constant pressure. During the five years in which that curve was determined, there was always one man who did nothing else but keep the current constant. You can do that with a dynamo, but you cannot do it with storage batteries. I say you cannot—you cannot in practice; and any man who draws inferences as to the life of lamps from a dynamo and from a storage battery, without having done that—well, I do not take much stock in his inferences. I have had a good deal of experience with lamps on storage batteries, and I am perfectly willing to be criticised when I make the statement that the pressure on the storage battery, as ordinarily installed, varies, I think, five times as much as it does on the average dynamo plant, and that the life of lamps on a storage battery plant, as ordinarily installed and handled, is very much lower than it is on the average dynamo plant, due to nothing else than the simple impossibility of keeping their pressure anywhere. I mean that that is a defect in their methods more than anything else. They use their lamps and charge the battery at the same time, and while they have means of keeping their pressure constant, they do not do it.

MR. MAILLOUX:—I wish to observe that I am very sorry Mr. Howell did not get hold of the right battery. Probably that is the reason why Mr. Howell has found such a great difference. It was not the same battery, and it was not handled the same way. We have found that by using suitable precautions and devices, it is possible to secure as constant, if not more constant, electromotive force with a storage battery, as with any other device for producing electricity, so that there should not be any great difference due to that cause. I freely confess that I have had no facilities for making quantitative experiments or determinations by which I could get at the exact value of the difference to which I adverted; but it seems to me that there must be a difference there somewhere, and it would be very interesting to get at what it is.

MR. HOWELL:—The handling of it has got more to do with it than the battery has. It just occurs to me right here that I was sent to investigate the lamps in a storage battery plant. Of course I went into the place as a stranger and asked innocent questions. In this case that I speak of I got the freedom of the plant and went down and talked with the engineer. I said "Those lamps don't look very bright; do you call that a good light?" He says, "No; that isn't any good, but you ought to be here when the engine is running."

MR. MAILLOUX:—When you take into consideration that a storage plant that gives nominally about two volts per cell requires from two and four-tenths to two and one-half volts for charging, and multiply that by fifty cells, you can readily perceive why the light was so much brighter when the engine was running.

MR. HOWELL:—There is no question that the possibilities of the storage battery are very great. For laboratory work, where we want a constant current, there is probably no equal to it for a short time, or probably for any time; but for practical work, as storage batteries are installed commercially, it is very far from being a constant pressure.

MR. FRANKLIN L. POPE:—So far as I know, no accurate experiments have been made to determine the difference in life between lamps run at an equivalent candle-power with an alternating and with a direct current; but I may say, that while I was in England about two years ago, there was some conversation among a number of electrical engineers one evening, in which that subject was brought up, and—speaking of a plant that had been installed in Whiteley's large establishment at the West End and worked directly by an alternating dynamo—I think it was a Ferranti machine—the assertion was very positively made, that the lamps showed a longer life with an alternating current than with a direct current, and the reason put forward to explain it was, that with a direct current the hot carbon was somewhat in the condition of an electrolyte and was more apt to throw off a deposit and disintegrate than when the current was continually reversed. But I do not know whether there is anything in that or not. I simply give it as the conversation I heard at that time.

Mr. Howell:—I think an argument drawn from one case or a few cases of that kind is hardly to be depended on, because you take the identical lamps and put them on different plants run from straight or alternating currents you get different results due to different handling. It is the hardest thing that I have ever undertaken to compare the lives of lamps. Now, as showing the difficulty and the misleading effect of the local conditions in a plant, when we first started in the business, we made lamps, of course from ninety to one hundred and ten volts, and on our isolated plants we ran lamps from ninety-five to one hundred, most of them, and those lamps were all made at once. Now, the man who put in our plants and who is in charge of the work, found a good many plants giving good results and using ninety-seven volt lamps. He found ninety-six, ninety-eight and ninety-nine were giving bad results, and he drew the conclusion that the ninety-seven volt lamps were better than ninety-six and ninety-eight. He drew that inference from facts determined from two or three plants at each time. Now, the volts of the lamp had nothing to do with it. The conditions of operating the plant affected the lives in the various cases. If the man who was getting good results from ninety-seven and the man who was getting bad results from ninety-six were to change their lamps, they would get the same as they did before. The lives of lamps are affected by every condition of running. To draw conclusions from a few lamps is misleading.

Mr. Upton:—We had another instance of the finding of the relation of lives of lamps. This came from Shamokin, Pa. By breaking off the tip of a lamp under water, you can fill the lamp, and you will obtain a little bubble there. This man broke the tips of a large number of lamps and concluded the larger the bubble the longer the life of the lamp. This satisfied him until we showed him that on the size of the opening depended the size of the bubble, and when broken in mercury you cannot see the bubble at all. I think the question of alternating current and straight current in its effect on the life of lamps is an unsettled question and that there is not an authoritative answer to that as yet. All our experiments have so far indicated negative results. The life is the same whether the current be straight or alternating. I do not think there are any experiments as yet proving a material difference between the two, that is, where the conditions are thoroughly watched all through. With alternating currents it is very difficult to indicate the pressure.

Mr. F. L. Pope:—I quite agree with Mr. Howell that deductions drawn from a few experiments have no value whatever, and I merely mention it as current talk without taking very much stock in it myself. I should not be at all surprised to find, after a proper investigation had been made, that there was little or no difference.

Mr. Mailloux:—In the early experiments in car lighting, that were conducted by a company here with storage batteries, they very quickly found it necessary to increase the efficiency of the lamp, otherwise there would not be enough energy to make a round trip. At that time the lamps of two and one-half watts per candle-power were not in commercial use. They were not to be had. So this company applied to the Edison people to have some lamps made of two and one-half watts. The Edison company replied that they could make lamps of any efficiency and that they would be glad to make them, but that they would not guarantee any life at all. I was not connected with the company at the time, but I understand that is the way it happened;—they took the lamps and concluded that they would risk it; let them break. They thought that lamps were cheaper than current, especially when you have got to take current from storage batteries; and they put the lamps on. I know that some of those lamps were in use for over three months. There may be some of them still in use, and they are running very satisfactorily, and they not only get full pressure, but they sometimes get more when the battery is overcharged and care is not taken to maintain constant electromotive force. The lamps now used in car lighting are all of two and a half watts or less. Whether that throws any light on the subject under those conditions I cannot say. It may be that the

Edison company did not consider it wise for them to use that same lamp at such a high efficiency.

Mr. Birdsall:—As regards the life of a lamp when running above voltage, I have found plants which have run the old Edison lamp—a ninety-four volt lamp. They ran at one hundred and fifteen volts, and then they would give nine hundred hours life, and the men did not appear to think that there was very much lamp breakage even for that excess of voltage.

Mr. Upton:—As to these curves which have been put on the board to-night, Mr. Howell asked me whether we should publish these results. We discussed it and concluded it was better to publish what we could about the relations between economies and lives of lamps. The time, we concluded, had come, when the public should fully recognize that the mere claim of giving 2 watts or 2½ watts or 3 watts or whatever it is per candle, means nothing, until these claims have been verified by long practice, and we know in the lamp business that no lamps can be put upon the market, unless they have a large margin over the laboratory test, as otherwise your complaints and troubles will more than counterbalance the gain, and these curves that Mr. Howell has presented here, are based, I think, safely on commercial practice, and we have chosen in this case about 3 watts per candle as our normal, and we recommend it as embodying our experience and the recommendation of the Edison Company for the best commercial practice.

Mr. Mailloux:—I would like to ask Mr. Howell if the light tests are made with a lamp running continuously; that is to say, is the current interrupted over night?

Mr. Howell:—Yes, sir; we shut down at six o'clock.

Mr. C. J. Field:—With regard to the new lamp mentioned there (the 3.1 watts lamp) I would state that it has been in use now since the first of last September. It is running, as a rule, 3 to 5 volts above pressure, and is run under all sorts of conditions. It has been shown on careful investigation by a man detailed especially for that purpose, that the new lamps are giving an average of over 1,000 hours' life in commercial use, and in some cases from 1,500 to 1,800 hours, and many agents are requesting the company to raise the guarantee to 1,000 hours.

Mr. F. L. Pope:—Mr. President, there is a great deal of uncertainty in the photometric measurements of lamps. Two or three years ago we heard a great deal about the remarkably efficient incandescent lamps that were produced in England, and a gentleman of my acquaintance procured some and undertook a photometric investigation of them, but he was not able to get as good results as they had obtained in England. As it happened, I was over there not long after and made some inquiries, and found the trouble to be in the system of measurement. My friend had measured them all around the horizontal circle, while the English photometers had taken merely the highest point. They had measured them at but one angle, and that was the way they obtained the results which were claimed.

THE PRESIDENT:—We have reached the usual hour of adjournment. Should there be no further discussion on this very interesting and valuable paper, a motion to adjourn will be in order.

I think we all feel greatly indebted to Mr. Howell for the trouble he has taken in preparing this paper, and also for the fact, that he has been enabled, and I may say allowed, to present to us information which has been accumulated at so great cost and expense and which must be of the utmost benefit to all who are engaged in the business and profession of incandescent lighting in this country and abroad. I am sure that these results will be noted with interest not only in America, but throughout the world, wherever incandescent lamps are used.

The knowledge of the fact of Mr. Howell's constant application to the study of the incandescent lamp for the last five years, and probably longer, was to my anticipations a sufficient guarantee that his paper would be of great value. We expected much from him, but he has given us even more.

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, June, 1888. No. 9.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

General Meeting, Wednesday, May 16th, 1888.

The General Meeting of the Institute was called to order at 10.30 a.m., on May 16th, by Mr. T. C. Martin, Vice-President.

The Chairman:—As you are aware, gentlemen, Mr. Edward Weston was elected President of the Institute last night. I have been informed that he is not at all well, and that it is possible that his sickness may, while not preventing his attendance, delay him very much in getting here this morning. I think it would be well, however, if we were to proceed immediately with our programme, and I will therefore present, as first upon the programme, the paper by Mr. P. B. Delany, on the "Protection of the Human Body from Dangerous Currents;" and as I have promised Mr. Delany to read that for him, I will ask Prof. Anthony if he will preside while I do so.

Prof. Anthony took the chair, and Mr. Martin read Mr. Delany's paper.

Read before the twenty-third meeting of the Institute, New York, May 16, 1888.

PROTECTION OF THE HUMAN BODY FROM DANGEROUS CURRENTS.

BY PATRICK B. DELANY.

Since the object of reading a paper before the Institute should be the enlightenment of members on the subject under consideration, by the presentation of facts developed by experimentation or of theories supported by a reasonable amount of investigation, I must apologize for my delinquency in both respects, and trust to the kind indulgence of the society in making a few suggestions, with a view of eliciting information and opinions from those whose experience and familiarity with the subject enables them to speak with authority.

I allude to what are considered dangerous currents and their effect on the human body. That there is danger in handling conductors conveying high tension currents must be generally admitted. The question, therefore, is, can fatal results from such currents be wholly or partially averted? All will agree that with proper care on the part of the companies and intelligent co-operation on the part of employés, but little would be heard of the danger of electric currents. But companies are economical and employés are careless. Familiarity with danger breeds contempt for it. This is exemplified almost daily in every department of industry. A new man in a powder mill will exercise the greatest care and observe all the rules of the establishment faithfully. In a few months the same man will smoke a pipe over an open powder keg.

Protection is necessary principally for those who through carelessness or ignorance fail to protect themselves. It has been claimed that there can be no such thing as an accident—that nature has none, and that all so-called accidents are simply the results of miscalculation, bad judgment or carelessness. But it must be remembered that the vigilance of the most alert will sometimes relax, and this to them is the moment of danger.

Realizing that something should be done to protect dynamo men and wire handlers from injury from powerful currents, I, a few years since, invented a device for shunting the vital parts of the body. Partly on account of a pressure of business in other branches of electricity, but principally owing to a hesitancy in bringing out a device about the efficacy of which there seemed to be some doubt, its introduction to scientific criticism was delayed until a few weeks ago. As the references made to the question in the electrical papers have failed to elicit an expression of opinion satisfactory to any degree, it has been deemed advisable to bring the subject before this meeting.

The question to be determined is, to what extent a wire of practically no resistance, extending from one wrist to the other and bound around the wrists in numerous convolutions, so as to make good but not uncomfortable contact, will protect the vital parts of the body from fatal injury when it is placed in the path of a dangerous current through contact at the hands. It is claimed by some that the current having once entered the muscles, veins and tissue at the hands, will not leave the arms at the wrists and follow the shorter path of the wire, but will confine its course to the arms and body alone. The other opinion is that the greater portion of the current received at the hands will leave the arms at the wrists and follow the wire. The problem seems a difficult one to solve. So much depends on the condition of the person at the time of receiving a shock, a large number of tests would be necessary to arrive at results which would afford a safe basis for calculation. A few years ago Dr. Stone, connected with one of the London hospitals, made a series of interesting experiments upon patients afflicted with various ailments, with a view of determining whether or not fluctuation in their physical condition affected the electrical conductivity or resistance of their bodies. The results proved, according to my recollection, that great changes took place in the electrical resistance of the patients experimented upon from day to day, and that there was a direct influence manifested by the rise and fall of different diseases upon the electrical conductivity of the body as a whole or in part. An account of Dr. Stone's experiments was published in *Nature* at the time. But while the experiments were most valuable for the purpose for which they were intended, they throw but little light on the subject of dangerous currents and protection from fatal injury.

Last September there was published in the *Electrical Review* of London, a paper on "Shocks from Alternating Currents," by Mr. G. L. Addenbrooke, who for some time previous had charge of the Grosvenor Gallery installation, where a primary current of 2,500 volts was constantly employed. While no one had ever come in contact with both mains at once, the writer stated that even if such an accident had happened he was "by no means prepared to assert that the result would undoubtedly be fatal." "Indeed," says he, "I rather incline to the opinion that if the subject of the shock were healthy, and the circumstances under which he received it were favorable, the chances are he would survive."

I cannot make out what Mr. Addenbrooke means by receiving 2,500 volts under favorable circumstances. It seems to me, if they are received at all, the circumstance could not be other than unfavorable. Mr. Addenbrooke himself received a severe shock, and as he speaks from experience, I quote his account of how it affected him: "The primary effect of the passage of the current through the body is, of course, to contract the muscles. The amount and persistence of this contraction depend on the quantity of the current flowing, or, as it is usually designated, *the intensity* of the shock, and varies from a simple twitching of the limbs to complete and intense contraction or rigor of apparently every muscle in the body. In this latter case, the current completely supersedes the action of the mind over everything capable of contraction or movement. Consciousness, however, is apparently rarely, if ever, lost, though the subject may be entirely unable to make any motion, cry out, or do anything to help himself. If aid is not near, such a state may be maintained for some time. On release from such a position by extraneous or natural causes, the subject is usually dazed, but if at all unconscious, soon recovers completely and experiences no further functional disturbance than the debility and nervousness naturally following such a severe strain on the system." While admitting that fatal shocks are possible, this gentleman gives it as his opinion that "if the shock is not sufficient to cause instantaneous collapse, apparently the body can bear the strain of the continued passage of the current for some time," and that death is caused by paralysis of the involuntary muscles controlling the heart and lungs, and that the preliminary stages of total collapse are analogous to what occurs when drowning." Referring to the local action of the current on the body where it enters and leaves, this writer also says: "If the contact is good, that is, if it consists of a good area, say of over a couple of square inches of a fairly moist skin pressed with moderate firmness against a metallic body or other good conductor, no visible injury may result. For instance, a man will often get burnt about the hands, but his feet, by which the current leaves, will be unaffected, or only slightly reddened or inflamed. Burning of the hands or other parts of the body will occur when there is imperfect contact of the body with the conductor, coming in contact with a bad or semi-conductor, such as a dirty or carbonized surface. The burning is caused by the absolute setting up of an arc."

It seems to me this gentleman's views, based, as we are led to believe, on actual experience, are somewhat paradoxical. And surely numerous cases of deaths from currents recently, disprove many of Mr. Addenbrooke's deductions. True, nothing short of death is made public, so that there is but little information to go upon relating to the instances which must be of frequent occurrence where men are *almost* killed by electric shocks. In all cases that have come to light, whether simply injuries or cases of death, the parts of the body coming in contact with the current have been badly burned. The unfortunate "trouble hunter," who lost his life a few days since, was not only burned at the hands but at the knees. In cases where the shock is not instantly fatal, may it not be that the burning of the flesh at the points of contact is so great as to destroy to a considerable extent the conducting substance in contact with the wire, and thereby cause a fall in the current passing through the body? It is certainly owing to the fact of the body being a very poor conductor that harm is done. If it offered no resistance there would be no realization that a current was passing through it. An arc would be created at the point of contact, provided the contact was *imperfect*, but the body itself would be uninjured. It seems to me that the human body must be viewed as any other semi-conductor, with the exception perhaps of susceptibility to greater change under the continued action of a current owing to the fluids which it contains. It is owing to its resistance that a carbon filament becomes incandescent.

The carbon rod of an electric lamp is not consumed except at the point where the arc is formed by the resistance of attenuated particles under combustion. Any conducting substances placed in the path of more current than it is capable of conveying must be heated, consumed or decomposed, according to their nature and in proportion to the discrepancy between the conducting capacity of the conductor and the sum of the current.

Now if we liken the human body from hand to hand to a canvas hose or tube filled with water and which allows a certain quantity of moisture to ooze through the surface, corresponding to the moisture of the skin, and if this be inserted in a circuit, connection being made at the extreme ends, corresponding to the hands, and a shunt wire of minimum resistance be wound around the tube two or three inches from the ends, corresponding say to the wrists, we can, perhaps, more understandingly speculate on the action of a dangerous current on the human body, contact being made at the hands and the shunt wire encircling the wrists. It should be borne in mind that the contact between the shunt wire and wrists is much better than the contact at the hands, since the wrists are more tender than the hands. Now, referring to the tube, and assuming that the contacts at the ends have improved by reason of the currents eating its way into the water, does it not follow that the current instead of following the entire length of the tube of water, offering, say thirty times the resistance of the two or three inches of water from the end of the tube to the shunt wire, and several thousand times the resistance of the shunt wire itself, will, to a great extent, pass from the tube to the shunt wire and in its passage *improve* the contact at that point as it did at the ends in entering the tube? I have often noticed that upon receiving a shock of 150 to 250 volts profuse perspiration instantly followed. This would greatly improve the contact at the wrists above the normal, so that it would seem impossible that the current should confine itself to the fluids and muscles of the body in preference to the wire when separated from the wire only by thin, tender skin filled with moisture.

In considering the danger point of currents it seems reasonable to recognize the same law, or rather recognize the absence of any law, governing other destructive agencies. Let it be concussion, strain, asphyxia, poison, or other enemy of endurance, there are hardly two cases alike. One man may emerge with but slight injury from a cause which to another man would be death. Why should it not be so in the matter of electric shock? Death, of course, must be the consequence of a certain degree of cause. If 2,000 volts are necessary to kill in any instance, one volt less would avert death, while shunting 100 or 500 volts would leave a proportionate margin of safety. Fatal results through the medium of electricity do not come by jumps any more than by any other force or element of destruction. There is in this subtle agency, as in all others, a last straw which breaks the camel's back—a last volt which does the final harm. Hence the importance of protecting, as far as possible, those exposed to this danger.

Regarding the immediate cause of death or the effect of currents on the substances of the body, we must look to the medical men for light. Probably all subjected to shock might not be similarly affected. In one case it might be the heart, in another, the brain, and in others, the lungs that might be injured. Why not have a thorough investigation of this subject, and then perhaps wire handlers may be subjected to an examination before they are allowed to engage in such work, just as men are examined for various other duties.

There seems to be so little known about these matters, I shall feel gratified if this paper serves as even a feeble incitement to discussion and investigation, as I can conceive of no subject offering a more humane incentive.

DISCUSSION.

Mr. G. M. Phelps, Jr.:—I suppose it will be somewhat difficult to bring the merit of the proposed protection to a test by a trial by linemen, or upon any person. There will probably be few investigators enthusiastic enough to learn the possibilities of this protection by submitting to a test. But unless electricians share the sensibilities of the Anti-vivisection Society, a great deal might be learned as to the efficacy of the means of protection suggested by Mr. Delany by judicious experiments upon animals. I suppose there will be an opportunity to get an abundant supply of dogs of various sorts the latter part of the summer. I merely make that suggestion as pointing out a method of testing the efficacy of this means of protection. I am quite serious about it. I can see no reason why very satisfactory tests might not be applied to various animals, and some of these physiological questions implied in Mr. Delany's paper be determined to a great extent.

MR. MARTIN:—Mr. Delany was not at all anxious that the apparatus he had devised should be exhibited, not desiring in any way to use the Institute as an advertising medium. I was very much interested in the subject myself, however, and I took the liberty of bringing some of the apparatus with me. If it be the desire of the members, I will show it.

(MR. MARTIN exhibited the apparatus.)

MR. R. W. POPE:—Mr. President, one of the difficulties encountered in the practical application of a safeguard of this kind, according to my investigations, has been the difficulty in enforcing its use among the very men whom it may be intended to protect. I have heard it stated that miners were averse to carrying safety lamps, and we all know that there are insulated pliers now on the market, and that India rubber gloves when perfect, answer as a protection; yet men go out on their daily duties without taking even these precautions, and consequently we need something more than the actual invention of a device that will perform its duty in order to save lives. If men are so careless and so regardless of the danger as to go out on their duties without preparing against it, I do not see how we can do anything to wholly prevent accidents.

MR. JOSEPH WETZLER:—In looking at the question from another standpoint, it strikes me that the question which is involved in the fatality of an electric shock, depends to as great extent on the electric work done upon the body, as upon the potential. Let us take, for instance, the case of a man struck by lightning. We frequently hear of men having been struck and surviving. Here there can be no question of the fact that the potential was at least many hundreds of thousands volts. Yet on the other hand we see a man touching a wire of 1,500 or 2,000 volts and being killed. In one case we have an enormous potential, but an exceedingly small current; in the other case a comparatively small potential, but a large current; yet in the latter case the electric work far exceeds that of the lightning stroke and the man's death ensues. Therefore I should say that the question involved as much the electric work done upon the human body as the potential; and if by means of such an apparatus the work done in the human body can be decreased, I think it would have in that sense some value; that is, if you could shunt a large part of C, a man's life might be saved, whereas if it were not shunted it might enter his body and cause his death.

Prof. P. H. Vander Weyde:—We have lately seen in the electrical journals assertions about the number of volts of potential of current people can stand without injury. It has been stated that a thousand volts could be taken with perfect impunity. But those writers forget that there are two elements to be considered—the potential and the quantity, as stated by the last speaker. It is the product of amperes and volts which does the damage. When we have a great potential, as in a friction electrical machine; we may have, as Prof. Mayer, in Hoboken, lately showed, many thousands of volts potential but no quantity. It will penetrate a distance easily, but the quantity transmitted is very small, so that it is harmless. The sparks of a friction machine do not cause serious damage. But when we take the other extreme, larger quantity and greater number of amperes, it is different. We find that in the thermo-electric pile there is not sufficient electromotive force to penetrate the skin. It has no effect on the body. There is not enough electromotive force even to pass through the liquids of the body. In the galvanic battery, we have electromotive force if we use a sufficient number of elements and we have quantity if our cells are large enough. There we may have a shock, and a battery of that kind—as large as Sir Humphrey Davy used, of some four thousand elements—may be fatal to an experimenter. It is the relation between the two. There must be a sufficient number of volts and a sufficient number of amperes together, in order to destroy life. That is the case with lightning. Usually lightning is fatal, because there is sufficient quantity, and at the same time there is electromotive force, as we see by the enormous length of a stroke of lightning. So it is with dynamos; when high electromotive force is combined with sufficient quantity, it becomes dangerous; but I want to impress upon all, that electromotive force alone, without quantity, is harmless, and so, also, great quantity alone is harmless. It is the combination of the two in which lies the danger.

Prof. Elihu Thomson:—Mr. President, I quite agree with Dr. Vander Weyde in the opinion expressed that it is not a question of volts or current alone which determines the fatality of a shock. It is quite possible, no doubt, to take a charge through the human body equal to many thousands of volts. One may mount an insulated stool and be charged to a potential of ten or fifteen inches of striking distance, and have it discharged from him with no more discomfort than would result by the discharge through his body of a very small Leyden jar. That I have had experience in myself, and I can testify that even though such a charge as that goes directly across the body of a person it has no harmful effects, other than a very slight shock, causing a twitching of the arm. In all cases of dangerous shock it seems to me that we must have a resistance so low as to permit the volts to send a current of a certain volume through the system. If that current is not attained, it is doubtful whether harmful results will follow. If that current is attained, the harmful results may follow. If we could imagine a man constituted so as to have not more than an ohm resistance separating any two portions of his body between which are situated vital nerves—nerves whose action is required to keep up the functions of the respiratory organs or the heart—it is quite conceivable that a very small voltage might kill the man. Human subjects undoubtedly differ very greatly in resistance, and a fatal shock may be obtained in one case at one time, which would not be obtained by the same subject at another time with equal voltage, much depending on the moisture of the skin, and probably the amount of salty matter in the blood. The layer of fat which exists underneath the skin acts as a sort of insulator. All these things will have an influence, and then comes in addition the susceptibility to a discharge. We have reason to believe, I think, that susceptibility varies very greatly in people. I quite agree also with Mr. Phelps in the necessity of some really scientific investigations tending to clear up this question. Those investigations, I think, could be carried on with the lower animals, and very valuable data arrived at, as to the passage of the current in certain directions through the body; whether the current in its passing includes, say, the pneumo-gastric nerve, which, if it does not act, is sure death every time. The heart must beat in order that life be sustained, and death will occur from heart failure if the heart is not kept beating. If, in like manner, the nerves controlling the respiratory functions have their vitality exhausted, that will also be a cause of death. The whole organism depends, I think, upon the nervous system, and hence it is a possibility that such a thing as nervous polarization may occur. The molecules constituting the nerve fibres may become set or polarized, so that the brain's excitation cannot reverse them nor bring them back to a normal condition. This idea has been suggested as a means of resuscitation—a discharge passed in an opposite direction would, as it were, depolarize the nerves and assist recovery, followed by artificial respiration and other means at command. It has been stated, I think by D'Arsonval, that in his opinion the alternating current is not as apt to produce fatal results as the direct current, and particularly direct currents in the nature of sudden discharges or shocks similar to lightning discharges, in which a large volume of current is sent through in intermittent impulses or in one impulse of considerable power. He, I believe, holds to the opinion that in cases of injury by alternating currents resuscitation may occur, in many cases, by artificial respiration. If this is true, it would seem that polarization has something to do with it, which is prevented in the case of alternating currents, because they are continually reversed. The fact also is certainly true that cases of very severe injury are known, where men have been subjected to a very high potential current and all the evidences of the passage of a heavy current through their bodies have been found, and the men have not suffered any great inconvenience other than the burning of the hands, so that it would seem as though a critical condition existed in some cases which was not present in others. Whether the person himself has any extra power of resistance, or whether his nervous system is so toned up as to be capable of recovering after he has been partially injured is a question which would naturally arise in connection with this case. I cannot agree that it is a question of one volt more or less whether a man is killed or not. I think the time element is probably the most important of all. There are cases in which men have been struck by lightning, and, as you all know, have recovered. Discharges of potential of millions of volts have passed through their bodies and they have recovered, and in such cases the discharge was undoubtedly a sudden one, and the current passing must have been very considerable. I cannot regard a lightning flash as an example of a small current. The heating effects which are present in such cases are sufficient to show that the current may rise to very many hundreds of amperes. For example, if a lightning flash will deflagrate a small iron wire in a very small fraction of a second, it means that current, not volts, has done the work, and not only that, but a very large current. It may have existed for but a short time, but it certainly did exist, and the very suddenness of the action is in favor of very high current flowing. We have numerous instances of telegraph and telephone wires and other small conductors melted by the electric discharge from the clouds, and when we consider the conditions, we must see that the current is large. We have only to look at a lightning discharge to see that it may appear to us as thick, sometimes, as a telegraph pole. That, if it means anything, means a body of vapor highly heated, of some considerable area. But lightning discharges, according to my observation, are not always single and alone. They are sometimes repeated half a dozen times down the same path, and it is not to be wondered at that a man receiving, say, a discharge of half a dozen flashes down the same path in rapid succession, might be killed, whereas a man receiving one flash, might escape. I have noted that fact by simply keeping my head going during a thunder storm and keeping my eyes fixed in one direction, so as to catch the flashes, and they arrange themselves in parallel lines, oftentimes. I have counted as many as six in that way, very quickly following each other.

Dr. Vander Weyde:—I had an opportunity some twenty years ago of making experiments regarding the susceptibility of different persons to heavy shocks, when I was professor at the Cooper Institute. We had a Rhumkorff coil which gave sparks of twelve inches, and which, by some improvements in taking away all possible edges on the binding posts, I increased to 16 inches. In making the experiment I was a few times knocked down —not actually knocked down, so much as that I had to sit down; I could not help myself. I spoke of it to Mr. Kidder, well-known as a practicioner using slight electric shocks for medical purposes. He came to my place frequently, and I gave him advice about his machine. He said that the idea was exaggerated that such a Rhumkorff coil was so dangerous, and he was willing to take the strongest shock I could give. I said to him, "If you want to take a risk, I am ready, but bring witnesses." He came with witnesses, one of them being a reporter of the *New York Tribune*. I gave Mr. Kidder small shocks at first, gradually making them larger and larger, and he stood them perfectly and they did not knock him down. He had great power af resistance in that respect. The next day a sensational article was published in the *Tribune*, headed, "An Error in Science Exploded," and there it was said that all the talk that electricity could kill was nonsense; that the strongest machine which ever had been made was now at the Cooper Institute—that it was the strongest was proved by the fact that it cost not less than $400, and that Mr. Kidder, the well-known electrician, had stood all the shocks from it, and it gave a great many details which are very amusing to a man posted in electrical science, written by a reporter who was not familiar with the subject. It is strange to me that some persons have such great resistance.

Dr. Otto A. Moses:—The proposed experiments on animals would be very interesting, but it would be very difficult to make the conditions sufficient to satisfy all the requirements that are necessary for accurate experimentation. If you consider that the resistance of a man's body may vary under certain circumstances from ten or fifteen thousand ohms down to four or five ohms, it will be seen that it is necessary that the experiment should be methodically conducted. The skin is an excellent insulator, the blood an excellent conductor, the perspiration an excellent conductor. The varying pressure that we apply in coming in contact with a conductor is a cause of immense variation in the amount of current that passes through us. In fact, we may assume that our nerves convey impressions to us simply by the varying pressure that we give to an object, in that way bringing more or less vital electric action, or whatever it may be, into play. An interesting experiment will prove how variable the quantity of current, passed through a conductor, may be. At the time Mr. Edison was experimenting on a carbon telephone, he thought it essential, and it is essential that a uniform pressure should be upon the carbon buttons of the transmitters. All of his experiments were made under uniform conditions. For instance, in the ordinary carbon button, which was about one or 1½ centimeters in diameter, he applied usually a weight of one gramme, and he found then that they had a resistance of about one ohm, but varying hydrometric conditions affected the conductivity. A passing thunder-storm, causing the absorption of moisture by the carbon, produced a variation in the quantity of current passing through. So, without any apparent changes, we have evidently got changes of resistance. The skin varies in thickness, so that it is necessary in making experiments, to take one constant place and one constant thickness. It will be a very difficult thing to try the experiment, but still it would lead to good results.

With regard to the point that Prof. Thomson raised, that it was possible for a man to live after having received a severe stroke of lightning. That is quite true. But I think it may be ascribed to the fact, that in such a case the man's clothes would naturally be saturated by the rain and thus become excellent conductors. The man is struck, but the wet clothes serve to carry the current more than the body. There is probably an induced action that affects the man, but the skin, being an excellent non-conductor, furnishes such insulation that most all the current passes through the wet clothes.

With regard to the quantity of current that will affect the sensations, one or two interesting experiments may be not unacceptable. If you make a single battery of zinc and copper placed above and below the tongue, you will feel a slight shock, you will taste a slight acidity, and you will also see a flash which, with sensitive persons, becomes unpleasant. There you have an electromotive force of about a volt and a very small quantity of current. If you take through the temples 100 volts from a like number of ordinary Daniell's cells, you will get a very unpleasant stunning effect, but not a painful one. There you have the brain brought right in between the terminals; whereas if you were to take a shock from a Rhumkorff coil, such as Dr. Vander Weyde spoke of, where the spark was 16, 17, or 20 inches, you would be able to actually transfix yourself if you were of sufficiently high resistance. We have all seen blocks of glass, four or five inches in diameter, pierced by a spark from a Rhumkorff coil. So there, if the body is thoroughly insulated by the dry skin, and the spark not able to pass by mean of the skin, it might pass through the brain in that case and cause instant death. It would be very difficult to trace the passage of the spark, but I think death would be caused in that way. The location of the terminals, too, has very great influence on the sensitiveness. I myself am exceedingly sensitive to the smallest shock. It is so unpleasant that what could be borne by almost anybody puts me in a profuse perspiration. So that it varies immensely in individuals.

(Mr. Martin here assumed the chair.)

Mr. Francis B. Crocker read the following paper on "The Possibilities and Limitations of Chemical Generators of Electricity."

THE POSSIBILITIES AND LIMITATIONS OF CHEMICAL GENERATORS OF ELECTRICITY.

BY FRANCIS B. CROCKER.

Electro-chemistry seems less generally and less clearly understood than any other branch of electricity of equal importance. The facts concerning the dynamo, motor, telegraph and telephone are widely and definitely known; but there are ideas, statements and claims in regard to batteries which are further from the truth, more prevalent and go longer without being contradicted than any other collection of errors that I can call to mind.

Primary batteries have not received the attention in this country that they have abroad, particularly in England, where the successful or unsuccessful attempt to float a new primary battery company is a matter of almost weekly occurrence. Nevertheless the subject is a sufficiently important one, and the false ideas of which I have spoken have spread so far beyond the quacks, to whom such ideas are usually confined, that the subject deserves more study than it usually receives, particularly from the scientific standpoint.

Electromotive Force.—The electromotive force which a given battery or combination of materials will develop is the first and most important question in electro-chemistry. Of course the best and surest way to arrive at this E. M. F. is to determine it by actual experiment with a voltmeter, or, still better, by comparison with a standard cell. There are, however, many times when the materials or instruments are not at hand, and a calculation or predetermination of the E. M. F. is very convenient.

The formula for calculating the E. M. F. is obtained by assuming that the electrical energy of the given chemical combination is equal to the heat energy which the same combination is capable of producing, or $E\,C = 4.16\,C\,a\,H$, that is, volts multiplied by coulombs is equal to the coulombs multiplied by the electro-chemical equivalent, *i.e.*, the weight of material required per coulomb and that by the heat produced by one gramme of the material. The electrical equivalent of heat 4.16 is introduced to reduce both members of the equation to an electrical basis. Cancelling the C in both members, we have $E = 4.16\ a\ H$, in which E is the E. M. F. in volts, a is the electro-chemical equivalent (grammes per coulomb), and H is the number of heat units (gramme-degrees Cent.) produced per gramme of material by the given chemical combination. The values of a and H are given for most materials in electrical and chemical books of reference, and the calculation is a very simple one, nevertheless this formula and the principle involved in it are not generally understood and are very seldom used. The equation deduced above is that ordinarily given, but what I have found to be a more convenient form of it is obtained by assuming that $E = 1$, giving a its value for hydrogen (.00001035). Solving with respect to H we have $H = 23300$, which means that 23300 is the number of heat units per equivalent corresponding to one volt, and all that is necessary to find the volts of any given chemical combination is to divide the number of heat units per equivalent (which is the form they are almost always given in) by 23300 and the quotient is the E. M. F. in volts. For example, to obtain the E. M. F. which zinc and free chlorine will develop divide 48,600 (which is the number of heat units produced by the combination of one equivalent of zinc with one of chlorine as given by Thomsen) by 23,300, and the result 2.09 is certainly very close to 2.11, the actual value obtained by experiment. In the same way the E. M. F. of other combinations of materials may be predetermined, but it should be always borne in mind that the heat of combination corresponding to one *equivalent* of the material should be taken and not necessarily that corresponding to one atom; as for example in the case of zinc, the heat per atom (65 grammes) is 97,200 which has to be divided by 2 to reduce it to one equivalent because zinc is a dyad and takes two atoms of chlorine, which is a monad. In the case of monad metals like sodium, potassium and silver the figures may be taken just as they are given in the tables. Mistakes very frequently arise is this way; in fact I do not think I ever saw a table of electro-chemical equivalents in which this point was correctly introduced in every case.

In order to test the practicability and accuracy of calculating E. M. F., as well as to obtain reliable results and full data for their own value, I determined the E. M. F. given by thirteen of the most important metals in combination with free chlorine, bromine and iodine, and the results are given in tables A, B and C respectively. In table D, I have averaged the results and placed them side by side with the results obtained by calculation. The agreement between the two sets of figures is not perfect by any means, but when it is remembered how the E. M. F. of a cell will change one or two-tenths of a volt on apparently the slightest provocation, I think the figures are strikingly close, more so than I expected to find them. In more than half of the cases the difference is less than one-tenth of a volt, and the average difference is only slightly more than that There has been no warping or coaxing of the figures to secure uniformity. The results are given just as they came. If figures were picked out from the different observations, it would be possible to obtain almost perfect agreement between the calculated and the determined, as one may see by looking over the tables.

In regard to the experimental results as given in tables A, B and C, I may state that all the metals and salts used were chemically pure, with the exceptions of the metals magnesium and aluminium, and these were of a very good quality. Pure carbon plates were used for the negative plate, but platinum was also tried and gave substantially the same results. The E. M. F. was measured by a Thompson reflecting galvanometer (by Elliott) using two independent standard Daniell cells. One fact which strikes one in looking over the tables A, B and C, is that the E. M. F. is not very greatly affected by the solution; a certain metal gives about the same result in one chloride as in another, and the same is true of the bromides and iodides. In table E, I have given the E. M. F. obtained by substituting different metals for the zinc in a Daniel's cell, but these results being from only one series of observations are probably not so reliable as those in the preceding tables.

In the remaining table I have collected in condensed form the principal facts in regard to batteries in which zinc is used as the electro-positive material with the most important electro-negative or depolarizing materials. The facts in regard to metallic zinc, which hold good in all cases in which it is used (no matter what the other material is), are given on a horizontal line with zinc. Below this the facts in regard to each particular combination are given opposite the depolarizer. For example, the data of a zinc and chlorine battery are given on a horizontal line with chlorine, of a zinc and nitric acid (Bunsen) battery on a line with nitric acid.

TABLE A.

E. M. F. produced by different Metals in Combinations with Free Chlorine in Solutions of —,

	Magnesium Chloride.	Zinc Chloride.	Zinc Chloride.	Zinc Chloride.	Hydro-Chloric Acid.	Common. Salt.	Average.
Magnesium..........	2.83 2.65	2.65			3.2–3.15	2.85	3.1
Zinc................	2	2.20	2.13	2.11	2.20	2.18	2.11
Cadmium............		1.94		1.86	1.98	1.93	1.9
Aluminium..........	1.75	2.04	1.88		2.02	1.98	2.
Iron......		1.60	1.56	1.5	1.67	1.68	1.6
Cobalt..............		1.43	1.43	1.41	1.50	1.45	1.43
Nickel..............		1.33		1.27	1.40	1.33	1.33
Tin...............	1.50	1.70	1.6	1.57	1.68	1.66	1.61
Lead.......		1.63	1.62	1.60	1.68	1.65	1.63
Copper..............		1.31	1.30	1.29	1.37	1.34	1.32
Silver		1.11	1.12	1.13	1.15	1.10	1.11
Antimony...........		1.20	1.23	1.25	1.25	1.08	1.22
Bismuth.......		1.29	1.25	1.20	1.27	1.19	1.21

TABLE B.

E. M. F. of different Metals with Free Bromine in solutions of

	Magnes. Bromide.	Nickel Bromide.	Potass. Bromide.	Hydro-Bromic. Acid.	Magnes Br'mide weak.	Average
Magnesium............... .	2.55	2.45	2.51	2.7	2.57	2.56
Zinc....	1.84	1.78	1.82	1.79	1.85	1.79
Cadmium......................	1.6	1.54	1.62	1.57	1.61	1.58
Aluminium....................	1.5	1.46	1.47	1.46	1.6	1.53
Iron.................	1.22	1.25	1.31	1.26	1.33	1.3
Cobalt.........................	1.1	1.02	1.05	1.06	1.13	1.05
Nickel.........................	.8	.85–.8	.8	.87	.94	.85
Tin....	1.12	1.31	1.3	1.25	1.27	1.3
Lead...........................	1.4	1.27	1.33	1.28		1.33
Copper.........................	1.05	.96	1.01	.99	1.03	1.02
Silver..........................	.97	.94	.97	.95	.93	.95
Antimony......................	.99	.73	.71	.87	.97	.8
Bismuth........................	.92	.9	.96	.88	.92	.92

TABLE C.

E. M. F. of different Metals with free Iodine in solutions of —,

	Magnes. Iodide.	Zinc Iodide.	Potass. Iodide.	Average.
Magnesium	2.02		2.00	2.01
Zinc	1.35	1.23	1.29	1.25
Cadmium	1.12	1.12	1.13	1.12
Aluminium	.92	.83	.87	.88
Iron	.69	.6	.74	.68
Cobalt	.6	.42	.5	.51
Nickel		.35	.37	.36
Tin		.72	.69	.71
Lead		.82	.85	.83
Copper	.63	.67	.62	.64
Silver	.65	.67	.64	.65
Antimony	.41	.47	.46	.44
Bismuth	.39	.46	.42	.43

TABLE D.

Comparison of the E. M. F. Calculated from the Heat of Combination and the E. M. F. determined by experiment.

Metals.	Combining with Chl'rine Calc.	Determ.	Bromine Calc.	Determ.	Iodine Calc.	Determ.
Magnesium	3.24	3.1				
Zinc	2.09	2.11	1.68	1.79	1.05	1.25
Cadmium	2.	1.9	1.58	1.58	.97	1.12
Aluminium	2.3	2.	1.7	1.53	1.00	.88
Iron	1.75	1.6	1.5	1.3	.85	.68
Cobalt	1.64	1.43				
Nickel	1.57	1.33				
Tin	1.71	1.61	1.5	1.3		
Lead	1.76	1.63	1.38	1.33	.85	.83
Copper	1.4	1.32	1.07	1.02	.69	.64
Silver	1.25	1.11	.97	.95	.59	.65
Antimony	1.3	1.22				
Bismuth	1.3	1.21				

TABLE E.

E. M. F. produced by different Metals substituted for the Zinc in a Daniels cell.

Metals.	Volts.	Metals.	Volts.
Zinc	1.08	Lead	.58
Cadmium	.79	Copper	.07
Aluminium	.65	Silver	—.02
Iron	.64	Antimony	.09
Cobalt	.40	Bismuth	.17
Nickel	.1		

SYMBOLS. — Chemical expressions reduced to a common basis.	CHEMICAL EQUIVALENTS. — Proportions in which materials combine.	ELECTRO-CHEMICAL EQUIVALENT. — Weight of material consumed per amp. sec.	NAME OF MATERIALS.	Electromotive Force produced.	Weight of zinc consumed per Horse Power hour.	Weight of Depolarizer consumed per Horse Power hour.	Weight of Materials consumed per amp hour in each cell.	Cost of Materials wholesale.	Total Cost of both positive and negative materials per Horse Power hour.
	H=1.	milligrams per coulomb.		Volts.	Pounds.	Pounds.	Pounds.	Per Pound.	
½ Zn	32.5	.337	Metallic Zinc..............				.0027	$.07	
			used with following electronegative or depolarizing elements:						
I	127.	1.314	Free Iodine..............	1.2	1.67	6.53	.0105	3.50	$22.97
Br	80.	.828	Free Bromine..............	1.79	1.12	2.76	.0066	.38	1.12
Cl	35.5	.367	Free Chlorine..............	2.11	.95	1.04	.0029		
½ O	8.	.083	Free Oxygen*..............	1.9	1.05	.26	.00066		
½ S	16.	.166	Free Sulphur*..............	.95	2.1	1.03	.0013	.02	.17
			CHEMICAL COMPOUNDS.						
½ H_2O	9.	[illegible]	Water *..............	.5	4.	1.1	.00074		
HNO_3	63.	[illegible]	Nitric Acid..............	1.9	1.05	2.04	.0052	.06	.20
[illegible]	[illegible]	[illegible]	Chromic Acid..............	2.	[illegible]	[illegible]	[illegible]	.26	.28
[illegible]	[illegible]	[illegible]	Copper Sulphate anhyd.....	[illegible]	[illegible]	[illegible]	[illegible]		
[illegible]	[illegible]	[illegible]	Copper Sulphate Crystals..	[illegible]	[illegible]	[illegible]	[illegible]	.06	.56
[illegible]	[illegible]	[illegible]	Iron Per Chloride..........	[illegible]	[illegible]	[illegible]	[illegible]	.10	.74
Ag Cl	[illegible]	[illegible]	Silver Chloride..........	[illegible]	[illegible]	[illegible]	[illegible]	16.00	133.25
[illegible]	[illegible]	[illegible]	Mercury Sulphate..........	[illegible]	[illegible]	10.7	[illegible]	[illegible]	5.45
			MIXTURES.					Mixture per lb.	
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	.04	.27
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	7.04	[illegible]	.05	.42

* Calculated.

For convenience the table is divided into two parts; the left-hand side contains the purely scientific data, and the right-hand portion contains the practical facts.

In the first column of the first part is given the chemical symbols reduced to a common basis, any one of the expressions being chemically equivalent to any other. For example, one-half atom of zinc ($\frac{1}{2}$ Zn) requires one atom of chlorine (Cl), or one-half atom of oxygen ($\frac{1}{2}$ O), or one-third molecule of chromic acid ($\frac{1}{3}$ Cr_2O_3), or one-sixth of the mixture of one molecule or potassium bichromate and seven molecules of sulphuric acid ($\frac{1}{6}$ [$K_2Cr_2O_7+7H_2SO_4$]), and so on.

In the second column I give the chemical equivalents which are simply the proportions by weight required of the different materials. That is, when we use 32.5 grammes of zinc (hydrogen being assumed as one gramme), we require 35.5 grammes of chlorine, or 33.4 grammes of chromic acid, and so on.

The third column contains the electro-chemical equivalents, which are really exactly the same and directly proportional to the combining weights in the previous column, but are reduced to an electric basis of so many milligrammes per coulomb. To convert these figures into grammes per ampère hour simply multiply by 3.6.

In the first column of the second part of the table is given the E. M. F. of each combination, all of which with the exception of oxygen, sulphur and water are determined by experiment, most of them being well known and accepted values. In the case of oxygen, sulphur and water the E. M. F. is calculated as explained above and is merely given for comparison. No actual battery corresponding to these materials exists.

In the second column on the right-hand side, is given the number of pounds of zinc required per horse-power hour in the case of each combination. For example, a zinc and copper sulphate (Daniell) battery, requires 1.86 pounds of zinc per horse-power hour of electrical energy produced.

In the next column I give the weight of depolarizer required per horse-power hour in the case of each combination. For example, a battery requires 5.05 pounds of the mixture of three parts of potassium bichromate, and seven parts of sulphuric acid, which is about the ordinary strong bichromate solution for porous cell batteries. Of course the water required for the solution is not considered here or in any of the other figures of the tables, is about as high on the electro-positive scale as it is safe to go so long as water is present. Even in the case of zinc its affinity for oxygen is greater than that of hydrogen, it is therefore only by tolerance, so to speak, that zinc remains passive in the presence of water. If it is not properly amalgamated, or if the solution is too strong, local action does occur. If, in place of zinc, we use any more electro positive metal these troubles are aggravated; with magnesium, for example, there are only very few solutions in which it will stand without great local action. Metals of still higher power, such as sodium, cannot be used at all in presence of water. Jablochkoff has, I believe, made cells in which sodium is used with damp cloth, but this can hardly be considered as water.

The proper way to use metals of great chemical energy would seem to be with liquids which do not contain any oxygen, but unfortunately such solutions are not generally good conductors. Another possible plan is to employ a fused electrolyte, but this involves the serious difficulty of maintaining it in the fused state.

So far as I can see, zinc is a very satisfactory positive material for batteries. After all it is cheaper than any other metal except iron. It is a sufficiently good conductor and less dirty and liable to corrosion than almost any other metal; it is also, as I have just shown, about as high on the electro-positive list as is safe to go.

The electro-negative materials on the other hand leave far more to be desired. They are generally expensive, very troublesome to handle, and introduce that quality of simple dirtiness, to which no other name can be applied, and which is really the most serious objection to batteries. The electro-negatives will, however, do the work, and as we have seen it actually requires very little weight to give a great deal of power, if the materials could only be used in a more perfect manner than at present. That seems to be the thing to hope for. Another serious difficulty with batteries is their high internal resistance. A dynamo can easily be made to give 100 volts with a thousandth or a few ten thousandths of an ohm internal resistance, hence their great output; but a primary battery capable of giving 100 volts with only one thousandth of an ohm internal resistance would probably fill this house.

In this connection it will be interresting to consider how many cells of ordinary gravity battery are required to give one horse-power of current. Each cell will give one volt and not more than half an ampère under normal conditions, hence each cell furnishes half a watt or about 1,500 cells to the horse-power! These cells would, however, give that power for a long time. But the fact remains that this very large number is required to produce the power which a dynamo not much larger than one's hat will generate.

In conclusion, I would say that I am by no means a skeptic in regard to chemical generators of electricity; the possibilities are very, very great, as I have shown. But these possibilities do not seem to have been brought to reality in a very perfect manner as yet. But batteries, even in the imperfect state in which they exist to-day, have their useful and legitimate function. A Leclanché cell is exceedingly well adapted to ringing electric bells intermittently and to telephone work, and gravity batteries have long done good service for telegraphic purposes.

But when it comes to developing any considerable amount of actual power, then the limitations become apparent. When we remember that battery electricity will certainly cost in practice 50 cents per horse-power hour, since the materials alone cost 25 cents, and that dynamo electricity only cost 2 cents per horse-power hour, the claims of some primary battery electric lighting promoters show up in their true light. As a luxury, of course, it makes very little difference what it costs, but even then people soon tire of paying very high prices for that kind of a luxury. For small electric lighting and small power in special cases batteries are useful, particularly where no other source of current is available. A physician or dentist to whom a horse-power hour may be worth hundreds of dollars could almost afford to use a chloride of silver battery and throw away the silver.

MR. CROCKER:—I do not wish to be understood as entirely skeptical in the matter of primary batteries. As I have said, the possibilities are almost infinite, but there are certain facts which cannot be overcome, which might as well be looked at straight in the face in the beginning, as after the company is formed and the failure occurs. There are certain legitimate applications of primary batteries even in their present state of imperfection. For example, the Leclanché battery is very satisfactory for ringing electric bells and for telephone work—we do not need a dynamo for those purposes—and even for small power uses, although I have not yet seen the battery which will practically generate electricity for actual power purposes at a reasonable

price. As a luxury a battery can be used, or for some
special purpose, like a dentist's or a physician's work, wh
horse-power hour may be worth $1,000. There one could
to use the silver-chloride battery and throw away the s
(Applause.)

DISCUSSION.

DR. MOSES:—I think that we are very much indebted to
Crocker for his able paper, and we cannot too highly laud
expenditure of time, the energy and self-abnegation devot
preparing a paper of that character for the Institute. Ther
some questions he will permit me to ask him that will le
others, and in that way to a useful debate. I notice that in
table D you have used chlorine, bromine and iodine for the
elements that you combine. Why did you select those w
atomic weights are perhaps less accurately determined, and w
are not absolutely considered as elemental in their chara
Why did you not, perhaps, take oxygen or even carbon for
purpose?

MR. CROCKER:—In the first place, as I have said, oxygen
and simple, does not combine directly with zinc in a battery.
oxygen will dissolve only in the solution to a very limited ext
and it would, of course, be possible to use it, but only in a
delicate way. Whereas, bromine, chlorine and iodine will fr
dissolve in the solution, and thus can be practically used in
elementary state. So far as the elementary character of brom
chlorine and iodine is concerned, I have never heard it questio
before. If you mean that all so-called elements are not eleme
and that hydrogen is really the only element, why, then,
course, these three go with the others, but, so far as I know,
stand on the same basis as carbon, oxygen and the rest; a
furthermore, they constitute a series, or family, of chem
elements. The results obtained are more clear and more instr
tive when a series of elements are used than when you take
ments at random, such as the acids for instance, which are
exactly on the same basis. The real depolarizers are usua
elements like chlorine, bromine and iodine. Chromic acid its
is not a depolarizer, it is the oxygen it contains. In that c
it may be said that oxygen is employed as a depolarizer, but
is not free oxygen, it is in combination with chromide oxide, a
therefore the question is complicated. It is, however, perfec

possible to apply these same principles to any other chemical combinations, but it makes a more difficult problem, and the results are less clear.

Dr. Moses: The figures are very conscientiously given, and the statement which Mr. Crocker makes that he puts down here a negative quantity simply because his galvanometer shows it, is a very acceptable statement, because he shows what he gets. Even the most inaccurate observations or results, if the methods are given, prove of equal value with the most accurate, as we recollect in the case of Vauquelin's analysis in the beginning of this century, though done in the most imperfect way, according to present methods, are very valuable, because he gives an account of how he did it. But there is a series of facts that are brought out by an inspection of the table and that rather tend to point to the moral that chlorine is itself not an element. I would like to recall to Mr. Crocker's attention the fact that the very discoverer of chlorine doubted its elemental character for a very long time, and it is to-day a mooted question whether it is a simple element. You have in that table a series of comparisons in which you find, with but one exception, that the determination is less than the calculated one. I will read them: 3.24, 3.1; 2.9, 2.11; 2.19; 2.3, 2; 1.75, 1.6; 1.64, 1.43; 1.57, 1.43; 1.71, 1.61; 1.76, 1.63; 1.4, 1.32; 1.25, 1.11; 1.3, 1.22; 1.3, 1.21. That shows of itself there is a certain law there; it shows that perhaps you should assume a lower chemical equivalent for chlorine.

Would you also tell us why you prefer not to determine accurately the amount of water that you used for solution? Don't you find, since water is essential, that its chemical equivalent and its relations to the substances in solution should be just as accurately determined, though we are inclined to consider a solution is not a chemical combination? Still, for these purposes the elements go through combinations through the solution. Therefore, why isn't it essential to determine the water?

Mr. Crocker: The water is merely a medium for the action to take place in; that is really the chemical fact. Of course I could put there, in the case of the weights, about how much water an ordinary solution would require; but that really would be merely guesswork. The best you could do would be to find when you obtained the maximum results, and that would be a very difficult question, but I find that it makes very little difference in electromotive force whether a strong or a weak solution is used. It does make some difference. The stronger the solution, the less the electromotive force. The difference amounts to about 5/100 or 1/10 of a volt. This is because there is more tendency for the metal to go in solution when the latter is weak; whereas if the solution is already satisfied it has less affinity for the metal. That is the way I explain the fact. I intended to bring out this point in my paper, but it involves another series of experiments, and the ones I have given were all that I had time to undertake.

Dr. Vander Weyde:—Is it not the fact that a certain concentration of a solution gives the least resistance? We all know that, for instance, a mixture of sulphuric acid and water of a certain proportion is the best conductor. If we increase the water or the sulphuric acid the resistance becomes greater.

In the meantime, Mr. President, while I am up, I cannot refrain from paying a compliment to Mr. Crocker (applause) for his very useful labor. It was just the thing that was needed, and I rejoice that the young man goes in the right direction. It is an investigation in which I had been engaged myself some years ago, intending to publish some data, but other business intervened and I found the field so extensive that I despaired of making it as complete as Mr. Crocker has done. (Applause.)

I wish to make one remark about the function of water in the battery. I would like to hear Mr. Crocker's opinion about a widely adopted theory that the water is the main agent in the batteries—that it is the decomposition of the water which does the work; that the oxygen of the water combines with the zinc and that hydrogen is developed, and a layer is formed which is the cause of polarization, and in order to get rid of that hydrogen the depolarizers are applied. I would like to hear Mr. Crocker's opinion about that theory.

Mr. Crocker:—As regards the function of the water in the battery it is one of those metaphysical points. (Laughter.) The water may act and give up its oxygen. The hydrogen may give up the oxygen on one side and turn around and get more on the other side. So far as I can see, it is water all the time. It is not changed, and it acts as a transmitter; that is the most it does. It acts as a vehicle. But after the action is over, there is just as much water there as there was before, unless it has evaporated; and in fact some chemical actions involved in batteries produce water, and in that case there would be more water at the end of the action than there was at the beginning. Therefore, it could hardly be said that it was used up. Furthermore, the action would take place exactly the same in the anhydrous state, whenever it is possible to get that state. For example, with a fused salt the action is exactly the same. The zinc would combine with chlorine in fused chloride of zinc, as I have myself proved by experiment, and would give substantially the same electromotive force as it does in the presence of water. Water is merely a means of getting a liquid conductor, which it is necessary to have as an electrolyte. An electrolyte must be a liquid, and you can obtain the liquids by fusing a solid electrolyte, or by dissolving a solid electrolyte. Therefore I always look upon the water merely as a means, or medium, in which the action takes place.

DR. VANDER WEYDE:—But the question naturally follows that if the zinc is oxydized by the oxygen what becomes of the hydrogen at the point of contact with the oxygen? Then there is more than a mere vehicle. We see that the hydrogen is developed at the platinum or silver plate.

PROF. NICHOLS:—I would like to supplement Mr. Crocker's statement by an observation in reference to the behavior of iron, and I presume what is true of iron will be true of other metals which form two salts. If you take iron and platinum in a solution which will produce a ferric salt, and let the cell stand, so that the iron in solution becomes surrounded with a film of its own salt, then the ferric action will gradually cease and ferrous action will take place. After the cell has stood, the electromotive force will rise about one-fifth, and you can instantly restore it to the condition it had at first. If you take a porous cell and separate the iron and platinum, put the oxidized solution in connection with platinum and then around the iron something which will produce the ferrous salt, you will find that that battery has about one-fifth more electromotive force than in the case of the ferric combination. It seems as though a more successful battery ought to be made with iron producing the ferrous salts. You gain almost enough to make up the difference between iron and zinc; but the behavior of iron is very unsatisfactory, and the reasons for it are not always very clear. It seems as though oftentimes it was due to the formation of oxidized films which run the resistance of the cells up to an enormous height. I have known the resistance of bichromate cells with iron positive plates to run up to several hundred ohms in the course of an hour or so by the formation, apparently, of an insoluble film of some form of iron which was almost a non-conductor. Perhaps iron, if worked out patiently, might give us a very cheap metal for a pole of a battery which would reduce the cost of galvanic electricity very considerably.

DR. VANDER WEYDE:—I want to say in respect to silver being an electro-negative, that Mr. Crocker is perfectly right in that.

DR. MOSES:—The remarks of Prof. Nichols recall to my mind some experiments which he perhaps has overlooked, but which he made quite a number of years ago, in which he used, if my memory serves, zinc in a sulphate of copper solution, where the change of temperatures produced considerable variations. I would like to inquire whether that was not due to the formation of salts of variable solubility around the electrode.

MR. NICHOLS:—It might very easily be.

MR. MAILLOUX:—I would like to ask what appears to be a somewhat pertinent question in view of the discrepancy observed between the calculated and determined electromotive force, and that is as to the standard which Mr. Crocker used; what value he accepted for it, and upon what authority, so that we may know what kind of a volt he employed.

MR. CROCKER:—That is an important point, of course, because everything depends upon it. I used constant Daniell cells, two independent ones set up separately, with chemically pure sulphate of zinc and sulphate of copper, and chemically pure metals; the solutions were half saturated. Then I further checked the results by two electromotive forces which I happened to know and had previously determined very many times, and those are the bromide and the iodide combination of zinc. The former combination in itself is an excellent standard cell, that is, free bromine and zinc. It gives an electromotive force of 1.79 volts. That is the result, I suppose, of a thousand experiments carried on with a certain object. Therefore in going through my experiments when I came to the combination zinc-bromide and found that it gave an electromotive force of 1.79 volts, I felt perfectly at home, because I had been there so often before, and it corroborated by standard. Then the combination of zinc and chlorine, 2.11 volts has also been very frequently determined. In the case of the Upward (zinc-chlorine) battery, which came out about a year and a half ago in England, the published electromotive force was 2.11 volts. So there were several corroborative facts. So far as my experience goes, a standard Daniell cell does not vary very much in electromotive force.

Prof. Thomson:—I would like to say just one word on this subject. It has been probably a common experience of those having Daniell cells in operation, that, if the solution is not kept up to a high degree of strength, oftentimes the upper part of the copper will be eaten out, and an electrolytic deposit of the copper made in the lower part of the cell where the solution is strong; or, in other words, here is an indication of a difference in the volts which carries on a sort of plating action on the copper plate itself. Whether this be due partly to the effect of the acid, or that the solution comes through from the porous cup and floats on top, causing a difference in the electrolytic condition of the fluids alone, setting up current, I do not know. But it recalls to my mind an observation several years ago on the effect of simple heating of the solution by putting at the lower part of a test tube a little spiral of copper wire and carrying out a connection by an insulated wire from the spiral, filling up the test tube partly with sulphate of copper solution; putting another little spiral near the top of the fluid, and then heating, we find a difference of voltage, and it amounts, I think, to something over one-tenth of a volt, or rather, something over one-twentieth of a volt, if I remember the result of the experiments correctly; showing that a solution of the copper will take place in the hot liquid, and a deposition of the copper in the cold liquid. In other words, the hot liquid is more of a solvent than the cold, although it is a sulphate of copper solution throughout. Whether Mr. Crocker has taken into account the temperature of the solutions in making these tests I do not know.

Mr. Crocker:—I made the tests at the normal temperature. It did not vary 5 degrees from 68; I do not think it varied more than three or four degrees. What Prof. Thomson says about a hot liquid giving electromotive force with a cold liquid, as it were, is true. In fact, Mr. Bradley has a battery which works on that principle, giving quite an electromotive force; I could not say exactly what, but quite a fraction of a volt, and it depends, to a certain extent, upon the heat of the solution; and that is a very interesting and possibly a productive field for investigation.

Prof. Thomson:—There is another point and then I am through. It is probably well understood nowadays that all the elements are not to be considered as real elements as we find them. I do not mean that they are decomposable into hydrogen, although I think that quite possible, and perhaps even to a far more primitive condition than hydrogen, if we could use forces which we have not at hand—I mean the celestial forces. I mean that each particle of copper known to us is a combination of copper with copper; that the atom really forms a molecule which is a real compound. Two atoms or more atoms may be found united, but it will certainly take something to decompose these molecules, even though they be combinations of the same atoms. I am struck with the results obtained with aluminium in the work of Mr. Crocker. We find actually that this metal, so hard to get at, is really capable of giving an electromotive force less than zinc; so that it would seem as though by the proper use of zinc in some way or other we might separate aluminium. In other words, its electromotive energy is very much less than zinc, and in some cases not very much different from iron. In the recently developed reduction of aluminium we find that carbon at high temperature reduced it with comparative facility, and it may be that after all, we have only to find, something which will fluidize or liquify the aluminium, and we would then find that even common materials like zinc or iron would separate from these combinations.

MR. CROCKER:—In regard to aluminium, the same thing struck me that Prof. Thomson points out. By my experiments aluminium is apparently less electro-positive than zinc. I do not think that this is actually the fact. I think there is some loss of electromotive force which occurs. In fact the calculated results are higher than those of zinc, and it may be that with aluminium there is a formation of insoluble compound on the plate. I am quite certain that my observed electromotive forces, although they are the actual electromotive forces given by a good piece of aluminium, are lower than the theoretical and lower than the possible electromotive force.

DR. VANDER WEYDE:—It may be useful to state here that twenty years ago I attempted to substitute aluminium for platinum in the Grove battery, on the principle that nitric acid does not attack platinum. The result was negative. I got scarcely any current at all. It appeared that aluminium had not the power to decompose the nitric acid and produce the nitrous acid fumes as with platinum in the Grove battery. It was a very carefully made experiment and impressed me very much.

NOTE.—The continuation of the Transactions of the General meeting will follow in the July number.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, July, 1888. No. 10.

GENERAL MEETING, WEDNESDAY, MAY 16TH, 1888.

(CONTINUED FROM JUNE NUMBER.)

THE CHAIRMAN:—The next paper on our programme to be read is that on "Compensated Resistance Standards," by Prof. Edward L. Nichols, of Cornell University.

It affords me the greatest pleasure to announce that our President, Mr. Edward Weston, who has been quite ill, has sufficiently recovered to be with us and is now here, and that he will preside immediately after lunch.

PROF. NICHOLS:—I would like to state that the experiments I am about to describe had their origin simply in the desire to find something that could be used for a low resistance shunt, or for a low resistance standard, and which would obviate the difficulty which every electrician meets with in electrical measurements on account of the influence of temperature upon conductivity. I suppose there is no correction which we are obliged to make which is so troublesome as this one of the temperature coefficient. If it were merely a question of change of the temperature of the laboratory, we could correct for that. We could determine our coefficient and make the proper correction; but whenever current flows it heats the wire to a temperature higher than that of the room, and in the case of heavy currents this difference of temperature will be a large one, and one which cannot by any ordinary means be accurately determined; and so there is always an element of uncertainty in the use of a coil through which a current flows, and this has proved so serious, as doubtless many of you know, as to lead to the discarding, for accurate work, of shunt methods of measuring heavy currents.

A paper read before the American Institute of Electrical Engineers, May 16th, 1888.

ON COMPENSATED RESISTANCE STANDARDS.

BY EDWARD L. NICHOLS.

The electrician has few more troublesome corrections to make in the course of his measurements, than those arising from the influence of temperature upon resistance. Were the heating of wires due solely to changes in the surrounding air, the matter would not be a serious one. Such changes could be reduced to a minimum by devices well known in the art of physical measurements, unavoidable changes could be determined by the aid of the thermometer, the temperature coefficient for the various elements of the circuit could be ascertained and proper corrections applied.

Since, however, heat is developed in every circuit in amount measured by the product of the current and the E. M. F. between the terminals, the temperature of a wire through which a current flows is always higher than the surrounding atmosphere. In the case of heavy currents the temperature of the wire may be very high, indeed. The difficulty of applying corrections for temperature is such as to lead to frequent rejections of methods which would otherwise be of great convenience, and might lead to results of the highest accuracy.

The discovery of a substance, the resistance of which would not change perceptibly under the influence of heat, would, it goes without saying, be of great value. Unfortunately all known metals are influenced in the same sense, nor has any alloy been found which is devoid of a temperature coefficient. The exhaustive investigations of the British Association Committee in this field are too well known to need recapitulation, and while some alloys have been recently produced which are much better than anything that committee was able to discover, even these forfeit much of their usefulness on account of their low conductivity.

One substance only, among the solid conductors of electricity, seems to offer a solution of the difficulty. Carbon, as has been shown by Matthiessen,[*] Siemens, and many later observers, differs from the metals in showing a decrease of resistance with rise of temperature. The amount of this change, which differs widely in the case of the various artificial carbons, is given in the following table.

TABLE I.

INFLUENCE OF TEMPERATURE UPON THE RESISTANCE OF VARIOUS VARIETIES OF CARBON.

OBSERVER.	Variety of carbon.	Temperature interval.	Mean coefficient per deg.
Werner Siemens (*Wiedemann's Annalen*, 10, p. 560.)	Gas carbon	0 to 200°	.000345
	Pressed gas carbon	0 to 200°	.000301
Borgmann (*Journal der Russischen Physikalischen Gesellschaft*, 9, p. 163.)	Charcoal	26 to 260°	.00370
	Anthracite	20 to 250°	.00265
	Graphite	25 to 250°	.0008
	Coke	26 to 245°	.00026
Kemlein (*Wiedemann's Annalen*, 12, p. 73.)	Gas carbon (coarse)	18 to 200°	.000285
	Gas carbon (fine)	18 to 200°	.000287
	Carré's carbons	18 to 200°	.000321
Muroaka (*Wiedemann's Annalen*, 13, p. 307.)	Paris retort carbons	....	.000300
	Arc light carbons	....	.000405
	Arc light carbons	....	.000425
	Gaudoin's carbons	....	.000415
	Kaiser and Schmidt's carbons	....	.000370
	Heilmann's carbons	....	.000240
	Arc light carbons	....	.000156
	Siberian graphite	....	.000739
	Faber's lead pencil graphite	....	.000588

The temperature coefficient, while varying widely for different carbons, is very nearly constant for a range of more than 100 degrees, in the case of any given specimen. It seemed probable, in view of this fact, that a conductor might be constructed, by combining carbon with a metal, in proper proportions, in which the influence of temperature upon the carbon and the metal respectively would precisely counterbalance each other, thus securing to the combination a constant resistance throughout a considerable range of temperature.

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[*] Matthiessen. *Poggendorff's Annalen*, 115, p. 353.

The calculation of the proportions necessary to this result, in the case of carbon, the coefficient of which has been determined, and a given metal, copper for example, is very simple.

Assuming that the combination would be subject to the law of parallel circuits, no alloy being formed, and no chemical combination between the parts taking place, we have for the total resistance R:

$$R = \frac{R_m + R_c}{R_m\ R_c},$$

where R_m is the resistance of the metal and R_c the resistance of the carbon.

The equation of condition, for complete compensation will be

$$\frac{R_m'\ R_c'}{R_m' + R_c'} = \frac{R_m''\ R_c''}{R_m'' + R_c''}$$

where R_m' R_c' are the resistances of the components and R_m'' R_c'', the corresponding resistances at any other temperature within the limits of temperature for which compensation exists.

Now the variation of the resistance of a metal with the temperature may be expressed by an equation of the form:

$$R_m'' = R_m'\ (1 + at + bt^2),$$

where a and b are coefficients to be determined by experiment.

In the case of carbon, the coefficient a will have a negative value, and the equation will take the following form:

$$R_c'' = R_c'\ (1 - at + bt^2).$$

Between 0° and 100° the value of b in the case both of copper and carbon is very small. A recent determination of the coefficients for copper, made in the Physical Laboratory of Cornell University, yielded the equation:

$$R_m'' = R_m'\ (1 + .00380\ t + .00000017\ t^2).$$

The experiments, which covered a range of 100°, are in close agreement with the results published by Matthiessen.

In the above equation, for $t = 100°$, we have $bt^2 = .0017$. If we neglect the coefficient b and adopt for a the mean coefficient between 0° and 100°, we may write the equation in the simpler form,

$$R_m'' = R_m'\ (1 + .003847\ t),$$

which will give results agreeing with the complete form at 0° and 100° and will have a maximum error, at 50°, of .0008.

In the case of carbon the coefficient b may also be neglected without appreciable error.

A Carré pencil, measured by another member of the same aboratory, gave as mean coefficient between 0° and 100°, the value .000235.

We may therefore write the equation for this variety of carbon, as follows:

$$R_c'' = R_c' (1 - .000235\, t).$$

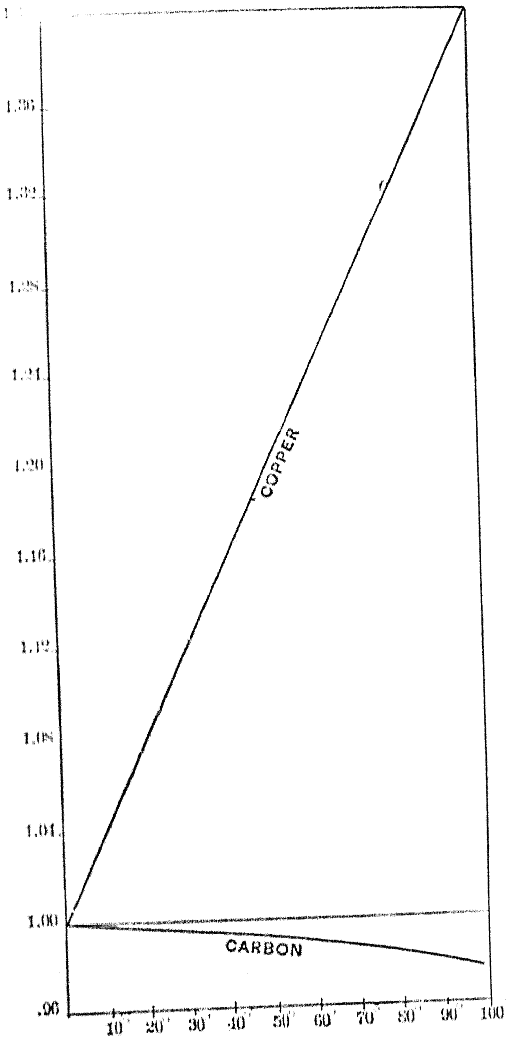


Fig. 1.

The relative influence of heat upon the resistance of copper and carbon is shown graphically in Fig. 1, which diagram will also serve to indicate the nearly constant value of the temperature coefficient of those substances.

In combining copper and carbon in such proportions that the resulting resistance shall be independent of the temperature, the previously stated equation of condition,

$$\frac{R_m' R_c'}{R_m' + R_c'} = \frac{R_m'' R_c''}{R_m'' + R_c''}$$

must be satisfied.

This equation will be satisfied only for a range of temperatures throughout, which b^2t is negligible, in the case of both substances.

For such a range of temperatures we have, however, as already indicated,

$$R_m'' = R_m' (1 + a_m t)$$
$$R_c'' = R_c' (1 - a_c t);$$

where a_c and a_m are the coefficients for carbon and copper respectively.

Within such limits

$$\frac{R_m' R_c'}{R_m' + R_c'} = \frac{R_m' (1 + a_m t) R_c' (1 - a_c t)}{R_m' (1 + a_m t) + R_c' (1 - a_c t)};$$

which is readily reducible to the form,

$$R_m' \left(\frac{a_c t}{1 - a_c t}\right) = R_c' \left(\frac{a_m t}{1 + a_m t}\right)$$

Using the coefficients already determined for copper and the variety of carbon in question, namely:

$a_m = .003847.$

$a_c = .000235.$

We find,

$R_m = 11.544\ R_c.$

In a word, we must use, for every ohm of carbon, 11.544 ohms of copper in order to secure complete compensation for temperature.

Three methods of combining the copper and carbon in such a compensated resistance have occurred to me as likely to give a good result. The first method consists in wrapping an insulated wire, of the necessary resistance, around the carbon and soldering the ends to the terminals of the bar, previously copper plated for that purpose. The second method consists in mixing the proper amount of copper in a finely divided state with the paste of which the rod is to be made, and then molding and baking the carbon in the usual manner.

In the third method, which is the only one which I have as yet put to actual trial, the carbon, having been tested and its temperature coefficient determined, is electroplated with copper. Experience showed that the film of copper necessary to compensation was so thin as to be scarcely adherent. The following process was therefore submitted for that of coating the entire surface. A Carré carbon 2.95 mm. in diameter was copper-plated for about 5 mm. at each end. The ends were then amalgamated and placed in mercury cups, which formed the terminals of a Wheatstone bridge. The resistance of the rod having been measured, and its mean coefficient between 20° and 100° having been determined, the length necessary for a compensated resistance of one legal ohm was calculated. The rod was then cut to the proper length, allowing 5 mm. at each end for terminals. The ends were heavily plated, and were inserted into the ends of copper rods, which were carefully soldered to the carbon. The rod was then given a coat of shellac, and, when dry, a strip of the varnish was removed with a sharp knife. The strip laid bare was parallel to the axis of the rod, and extended from terminal to terminal. Its width was about 1 mm. The rod was then submerged in a plating bath of sulphate of copper.

In this way a film of copper was deposited upon the exposed portion, which combined the required electrical resistance, 11,544 ohms, with sufficient thickness to render it tough and adherent. It was found that one gravity cell, acting through 50 ohms, would deposit the requisite amount of copper for a one ohm bar, in about ten minutes. When the above process had been carefully carried out, the rod was found to have very nearly the proper total resistance and to be nearly perfect in the matter of compensation. If the rod was over compensated, copper was removed by careful scraping. If it was under-compensated, carbon was removed by the same process. These final adjustments were continued until the rod showed no perceptible change of resistance when heated to 100°. When the compensation was regarded as satisfactory, the adjustment of total resistance was made by removing the copper-plating of the terminals, or extending the same by renewed plating. This, if carefully done, would bring the rod to the proper resistance without impairing the compensation. The rod, after adjustment, was well varnished, and was inserted in a glass tube of about 7 mm. internal diameter. This tube was provided with brass caps, through which the copper terminals projected to a distance of about 5 cm. These, after being securely riveted to the brass caps, were bent through an angle of 90°. When thus mounted the resistance-rod presented the appearance indicated in Fig. 2.

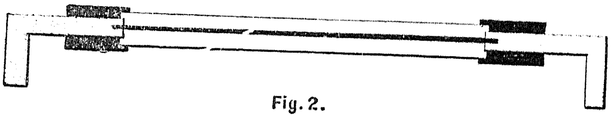


Fig. 2.

In the manner just described, I succeeded after a few trials in producing a resistance-rod upon which the influence of the temperature between 18° and 100° was entirely imperceptible. The object being to test the method of compensation, and not to produce a standard ohm, the adjustment for final resistance was not carried out.

The resistance of the compensated rod, after being mounted, is shown in the following table:

TABLE II.

RESISTANCE OF A COMPENSATED RESISTANCE-ROD AT VARIOUS TEMPERATURES; MEASUREMENTS BY MR. GEO. D. SHEPARDSON.

TEMPERATURES.	RESISTANCE IN B. A. OHMS.
105°C.	1.0300
98°	1.0300
87°	1.0305
35°	1.0305
22.5°	1.0295
19°	1.0300

For the purpose of this measurement, the rod was immersed in an oil bath and heated to more than 100°. The readings were taken while the bath was cooling. They indicate a practically perfect compensation throughout the range of temperature given in the table.

If such a compensated resistance-rod be quickly heated or cooled, the copper will change more promptly than the carbon and the balance will be temporarily destroyed. While in process of heating from without, the resistance will increase somewhat, and while cooling by radiation the reverse effect will be noticeable.

In rods not mounted in glass and unprotected by a coating of varnish these variations are very marked. Table III. contains a set of readings made upon such a rod. The rod was rapidly heated and cooled, and was measured several times during the rise and fall of temperature. The compensation was nearly complete, the mean coefficient between 20° and 100° being less than .00002, but the rod in question exhibited temporary variations when suddenly heated and cooled, amounting to more than one per cent.

TABLE III.

TEMPORARY VARIATIONS OF A COMPENSATED ROD SUBJECTED TO SUDDEN CHANGES OF TEMPERATURE.

Temp.	Resistance.	Temp.	Resistance.
25°	1.0050	78°	1.0030
(Gas ignited.)		(Gas extinguished.)	
37°	1.0072	78°	0.9965
45°	1.0074	74°	0.9960
55°	1.0070	63°	0.9975
62°	1.0040	40°	1.0030
68°	1.0040	25°	1.0050
70°	1.0040		

Compensated resistance-bars, such as have been described in this paper, are of course limited in their application to uses in which carbon conductors are admissible. For standards of resistance I think they will be found especially valuable. The convenience of having a comparison resistance the value of which can be determined once for all, and which can then be used with confidence, without regard to its temperature, even in circuits which would heat our ordinary standard coils and modify their resistances to a notable extent, will be appreciated by all. In the construction of low-resistance shunts, also, the possibility of obtaining a conductor which will remain unchanged at all ordinary temperatures, whether heated from without or by the passage of the current, will permit us to adopt with confidence the use of high-resistance galvanometers, with low resistance shunts, for the measurements of large currents, even where the highest accuracy is necessary.

To those who, in the course of their electrical work, have to cope daily with the question of the influence of temperature upon resistance, numerous other applications of compensated resistance-rods will suggest themselves.

DISCUSSION.

MR. WHEELER:—I would like to ask Prof. Nichols if he has any explanation of the sudden drop at about twenty-two degrees centigrade. I would also like to say that Prof. Thomson called my attention to the fact that a bar of this kind might be made in series by leaving the varnish complete all the way around it, then applying lamp-black or graphite, and plating on this a complete copper tube, and then connecting them at one end so that you could send the current through the carbon and back through the copper tube.

PROF. NICHOLS:—The only trouble is that it would be difficult to get a small enough amount of copper. You require eleven ohms of copper and that means a very small tube. This very slight change at 22° Cent., in my opinion, is not due to incomplete compensation but to accidental causes. It is very difficult to entirely get rid of thermo-currents. It is perfectly thinkable that there might be some irregularity in the temperature coefficient which would make the compensation slightly incomplete. At intermediate temperatures I do not think it is noticeable.

DR. MOSES:—By what method did you electroplate your carbon?

PROF. NICHOLS:—I put it into a sulphate of copper bath.

DR. MOSES:—I would suggest that you could avoid the use of paint by using cyanide of copper solution. It would be adherent and very dense.

THE CHAIRMAN:—The hour for lunch has arrived and I think we may therefore as well proceed to compensate our resistances according to our various standards. (Laughter.)

I would like to announce that the first paper which will be read immediately after lunch will be that by Mr. Tesla on alternating current motors and transformers. His paper is of a most interesting character.

AFTERNOON SESSION, MAY 16th, 1888.

THE CHAIRMAN: At the opening of our morning session, I had to announce that through serious sickness, our President, Mr. Edward Weston, who was unanimously elected last night was unable to be present. Mr. Weston has, however, made considerable effort to be with us to-day and he comes here from a sick bed. I think in securing the services of Mr. Weston in the presidency the Institute has secured one whose international reputation will be of benefit to it through the coming year. The Institute, as you are well aware, is a society whose membership is not merely confined to New York, but extends throughout every state and territory of the union, and includes to-day almost ever country on the civilized globe. Mr. Weston's reputation as an electrical engineer is co-extensive with the membership of the Institute, and I am therefore very glad to salute him as my successor, and now to introduce him to you.

Mr. Weston took the chair, and addressed the meeting as follows:

Gentlemen and Members of the Institute: I am not quite so sick as our former president, Mr. Martin, would lead you to believe; but since yesterday I have made a few trips between the equator and the north pole. I have been jumping as you will see between pretty wide extremes. To stop the too rapid motion I have taken considerable quinine and it leaves me in a somewhat befogged condition of mind. I am therefore not feeling well enough to address the Institute in the way that I should do in accepting the presidency. I feel extremely honored by the kindness of your selection. I accept the office with grave doubts, because I fear I cannot give as much time to it as our past worthy president has done, and show in the same time such remarkable growth of the Institute. I am informed that in the year of his presidency the actual membership of the Institute has about doubled itself, an indication of excellent work The papers that I have read, from what I have seen of the proceedings, have been worthy of the society. The character of the papers shows a constant upward tendency. I hope it will continue so. We have nothing to be ashamed of in the past, and I hope we shall find nothing to be ashamed of in the future. I had intended to speak in full of the present state and future progress of electricity. I amr unble to do that to-day. I really cannot stand it. Therefore I beg to apologize for disappointing you. I am not in fit condi- tion even to occupy the President's chair, and as Mr. Martin has suggested that he would rather withdraw to-day, I would like Mr. Upton, if he would kindly do so, to accept the position this after- noon. (Applause.)

Mr. Francis R. Upton took the chair.

THE CHAIRMAN:—The first paper this afternoon is on "A New System of Alternate Current Motors and Transformers" by Mr. Tesla.

MR. GEO. M. PHELPS, JR.: I would like about two minutes before the paper is read. I do not rise to propose a vote of thanks to our retiring President, because I think votes of thanks, like the resolutions in political conventions, "pointing with pride, &c.," have become a custom "more honored in the breach than in the observance." But I do feel that a word ought to be said, and I feel very confident that I express the general opinion in saying a word recognizing the zeal, the activity and the intelli- gence which have characterized the administration of the office of President during the past year. The growth in membership, the character of the work accomplished, and, in general, all the ac- tivities of the Institute during the past year constitute a record to which I think we shall always look back with satisfaction, and with reasonable and just pride. It has been a matter of astonish- ment to me to observe the amount of time and the intelligent effort which so busy a man as our ex-President has found it pos- sible to give to the interests of this Institute. He has not spared himself in any particular; there is no work that he could do that has been left undone, and our condition at the present time is a sufficient evidence of the truth of what I say, and I have not the slightest doubt that all here present join with me, and feel that it is no more than a meagre statement of the facts. (Applause.)

THE CHAIRMAN:—Is any formal motion to be made and entered in the minutes?

MR. PHELPS:—I think not, sir. As I expressly stated, I would prefer, as far as I am concerned, to avoid a formal motion. But the way in which my remarks were received sufficiently justified my assumption that they expressed the general opinion of the members present.

MR. MAILLOUX:—I would move that the remarks made by Mr. Phelps be heartily accepted and approved and endorsed by the society. The motion was seconded by Mr. Wheeler.

CAPT. O. E. MICHAELIS:—I would merely like to add one word to the encomium pronounced on Mr. Phelps. I have been fortunate enough to be associated with our past President in the Council during the past year and I can heartily endorse everything Mr. Phelps said; I would like, however to call the attention of the Institute to one fact to which they are indebted to our past President alone, and that is the valuable addition to our transactions in the shape of the bibliography that now appears with them regularly. That was Mr. Martin's idea, and it has been carried out by him. You all must know how much time such a thing has taken, and he has done that entirely of his own motion and for the good of the Institute. (Applause.)

THE CHAIRMAN: I understand the motion to be that the remarks of Mr. Phelps be added in full in our minutes and with the cordial approval of the members. All those approving will signify by rising. The resolution was unanimously carried.

MR. NIKOLA TESLA:—I desire to express my thanks to Prof. Anthony for the help he has given me in this matter. I would also like to express my thanks to Mr. Pope and Mr. Martin for their aid. The notice was rather short, and I have not been able to treat the subject as extensively as I could have desired, my health not being in the best condition at present. I ask your kind indulgence, and I shall be very much gratified if the little I have done meets your approval.

Mr. Tesla read the following paper.

A NEW SYSTEM OF ALTERNATE CURRENT MOTORS AND TRANSFORMERS.

BY NIKOLA TESLA.

In the presence of the existing diversity of opinion regarding the relative merits of the alternate and continuous current systems great importance is attached to the question whether alternate currents can be successfully utilized in the operation of motors. The transformers with their numerous advantages, have afforded us a relatively perfect system of distribution; and although, as in all branches of the art, many improvements are desirable, comparatively little remains to be done in this direction The transmission of power on the contrary, has been almost entirely confined to the use of continuous currents, and notwithstanding that many efforts have been made to utilize alternate currents for this purpose, they have, up to the present, at least as far as known, failed to give the result desired. Of the various motors adapted to be used on alternate current circuits the following have been mentioned: 1. A series motor with subdivided field. 2. An alternate current generator having its field excited by continuous currents. 3. Elihu Thomson's motor. 4. A combined alternate and continuous current motor. Two more motors of this kind have suggested themselves to me. 1. A motor with one of its circuits in series with a transformer and the other in the secondary of the transformer. 2. A motor having its armature circuit connected to the generator and the field coils closed upon themselves. These, however, I mention only incidentally.

The subject which I now have the pleasure of bringing to your notice is a novel system of electric distribution, and transmission of power by means of alternate currents, affording peculiar advantages, particularly in the way of motors, which I am confident will at once establish the superior adaptability of these currents to the transmission of power and will show that many results heretofore unattainable can be reached by their use;

results which are very much desired in the practical operation of such systems and which cannot be accomplished by means of continuous currents.

Before going into a detailed description of this system, I think it necessary to make a few remarks with reference to certain conditions existing in continuous current generators and motors, which, although generally known, are frequently disregarded.

In our dynamo machines, it is well known, we generate alternate currents which we direct by means of a commutator, a complicated device and, it may be justly said, the source of most of the troubles experienced in the operation of the machines. Now, the currents so directed cannot be utilized in the motor, but they must—again by means of a similar unreliable device—be reconverted into their original state of alternate currents. The function of the commutator is entirely external, and in no way does it affect the internal working of the machines. In reality, therefore, all machines are alternate current machines, the currents appearing as continuous only in the external circuit during their transit from generator to motor. In view simply of this fact, alternate currents would commend themselves as a more direct application of electrical energy, and the employment of continuous currents would only be justified if we had dynamos which would primarily generate, and motors which would be directly actuated by such currents.

But the operation of the commutator on a motor is two-fold; firstly, it reverses the currents through the motor, and secondly, it effects automatically, a progressive shifting of the poles of one of its magnetic constituents. Assuming, therefore, that both of the useless operations in the system, that is to say, the directing of the alternate currents on the generator and reversing the direct currents on the motor, be eliminated, it would still be necessary in order to cause a rotation of the motor, to produce a progressive shifting of the poles of one of its elements, and the question presented itself. How to perform this operation by the direct action of alternate currents? I will now proceed to show how this result was accomplished.

In the first experiment a drum-armature was provided with two coils at right angles to each other, and the ends of these coils were connected to two pairs of insulated contact-rings as usual. A ring was then made of thin insulated plates of sheet-iron and wound with four coils, each two opposite coils being connected together so as to produce free poles on diametrically opposite sides of the ring. The remaining free ends of the coils were then connected to the contact-rings of the generator armature so as to form two independent circuits, as indicated in figure 9. It may now be seen what results were secured in this combination, and with this view I would refer to the diagrams, figures 1 to 8*a*. The field of the generator being independently excited, the rotation of the armature sets up currents in the coils *c c*, varying in strength and direction in the well-known manner. In the position shown in figure 1, the current in coil *c* is nil while coil c_1 is traversed by its maximum current, and the connections may be such that the ring is magnetized by the coils c_1 c_1 as indicated by the letters N S in figure 1*a*, the magnetizing effect of the coils *c c* being nil, since these coils are included in the circuit of coil *c*.

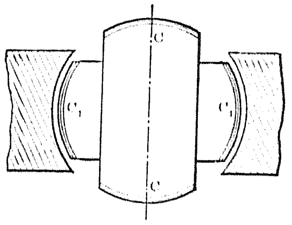


Fig. 1.

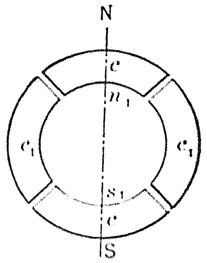


Fig. 1a.

In figure 2, the armature coils are shown in a more advanced position, one-eighth of one revolution being completed. Figure 2*a* illustrates the corresponding magnetic condition of the ring. At this moment the coil c_1 generates a current of the same direction as previously, but weaker, producing the poles n_1 s_1 upon the ring; the coil *c* also generates a current of the same direction, and the connections may be such that the coils *c c* produce

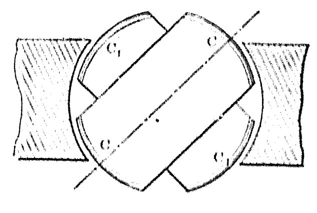


Fig. 2.

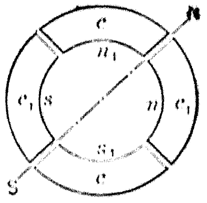


Fig. 2 a.

the poles *n s*, as shown in figure 2*a*. The resulting polarity is indicated by the letters N S, and it will be observed that the poles of the ring have been shifted one-eighth of the periphery of the same.

In figure 3 the armature has completed one-quarter of one revolution. In this phase the current in coil c is maximum, and of such direction as to produce the poles N S in figure 3*a*, whereas the current in coil c_1 is nil, this coil being at its neutral position. The poles N S in figure 3*a* are thus shifted one quarter of the circumference of the ring.

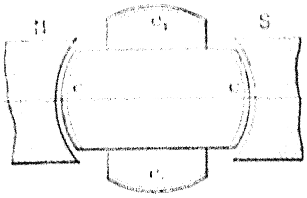


Fig. 3.

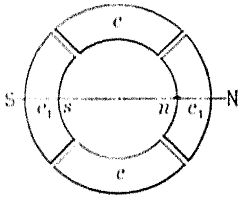


Fig. 3 a.

Figure 4 shows the coils c c in a still more advanced position, the armature having completed three-eighths of one revolution. At that moment the coil c still generates a current of the same direction as before, but of less strength, producing the comparatively weaker poles *n s* in figure 4*a*. The current in the coil c_1 is of the same strength, but opposite direction. Its effect is therefore, to produce upon the ring the poles n_1 s_1 as indicated and a polarity, N S, results, the poles now being shifted three-eighths of the periphery of the ring.

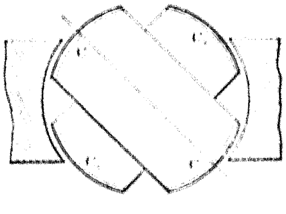


Fig. 4.

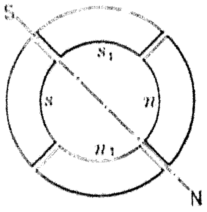


Fig. 4 a.

In figure 5 one half of one revolution of the armature is completed, and the resulting magnetic condition of the ring is indicated in figure 5*a*. Now the current in coil c is nil, while the coil c_1 yields its maximum current, which is of the same direction as previously; the magnetizing effect is, therefore, due to the coils,

c_1 c_1 alone, and, referring to figure 5*a*, it will be observed that the poles N S are shifted one half of the circumference of the ring. During the next half revolution the operations are repeated, as represented in the figures 6 to 8*a*.

A reference to the diagrams will make it clear that during one revolution of the armature the poles of the ring are shifted once around its periphery, and each revolution producing like effects, a rapid whirling of the poles in harmony with the rotation of the armature is the result. If the connections of either one of the circuits in the ring are reversed, the shifting of the poles is made to progress in the opposite direction, but the operation is identically the same. Instead of using four wires, with like result, three wires may be used, one forming a common return for both circuits.

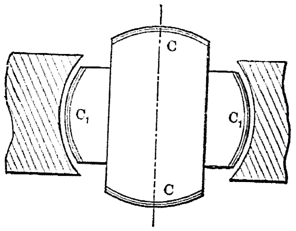


Fig. 5.

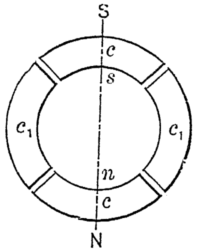


Fig. 5 a.

This rotation or whirling of the poles manifests itself in a series of curious phenomena. If a delicately pivoted disc of steel or other magnetic metal is approached to the ring it is set in rapid rotation, the direction of rotation varying with the position of the disc. For instance, noting the direction outside of the ring it will be found that inside the ring it turns in an opposite direction, while it is unaffected if placed in a position symmetrical to the ring. This is easily explained. Each time that a pole approaches, it induces an opposite pole in the nearest point on the disc, and an attraction is produced upon that point; owing to this, as the pole is shifted further away from the disc a tangential pull is exerted upon the same, and the action being constantly repeated, a more or less rapid rotation of the disc is the result. As the pull is exerted mainly upon that part which is nearest to the ring, the rotation outside and inside, or right and left, respectively, is in opposite directions, figure 9. When placed symmetrically to the ring, the pull on the opposite sides of the disc being equal no rotation results. The action is based on the magnetic inertia of the iron; for this reason a disc of hard steel is much more affected than a disc of soft iron, the latter being capable of very rapid variations of magnetism. Such a disc has proved to be a very useful instrument in all these investigations, as it has enabled me to detect any irregularity in the action. A curious effect is also produced upon iron filings. By placing some upon a paper and holding them externally quite close to the ring they are set in a vibrating motion, remaining in the same place, although the paper may be moved back and forth; but in lifting the paper to a certain height which seems to be dependent on the intensity of the poles and the speed of rotation, they are thrown away in a direction always opposite to the supposed movement of the poles. If a paper with filings is put flat upon the ring and the current turned on suddenly, the existence of a magnetic whirl may easily be observed.

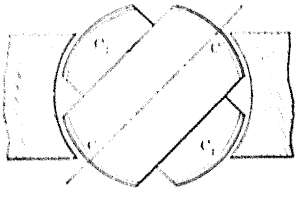


Fig. 6.

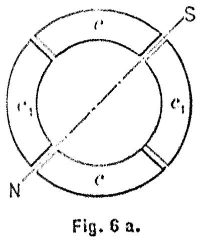


Fig. 6 a.

To demonstrate the complete analogy between the ring and a revolving magnet, a strongly energized electro-magnet was rotated by mechanical power, and phenomena identical in every particular to those mentioned above were observed.

Obviously, the rotation of the poles produces corresponding inductive effects and may be utilized to generate currents in a closed conductor placed within the influence of the poles. For this purpose it is convenient to wind a ring with two sets of

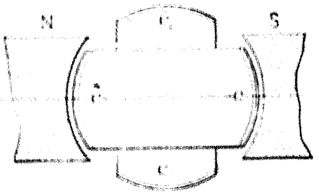


Fig. 7.

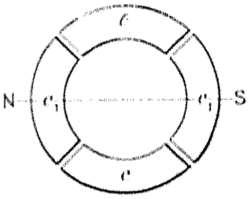


Fig. 7 a.

superimposed coils forming respectively the primary and secondary circuits, as shown in figure 10. In order to secure the most economical results the magnetic circuit should be completely closed, and with this object in view the construction may be modified at will

The inductive effect exerted upon the secondary coils will be

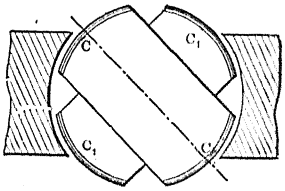


Fig. 8.

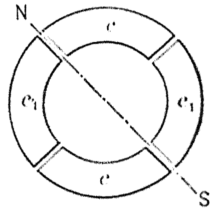


Fig. 8 a.

mainly due to the shifting or movement of the magnetic action; but there may also be currents set up in the circuits in consequence of the variations in the intensity of the poles. However, by properly designing the generator and determining the magnetizing effect of the primary coils, the latter element may be made to disappear. The intensity of the poles being maintained constant, the action of the apparatus will be perfect, and the same result will be secured as though the shifting were effected by means of a commutator with an infinite number of bars. In such case the theoretical relation between the energizing effect of each set of primary coils and their resultant magnetizing effect may be expressed by the equation of a circle having its centre coinciding with that of an orthogonal system of axes, and in which the radius represents the resultant and the co-ordinates both of its components. These are then respectively the sine and cosine of the angle α between the radius and one of the axes $(O X)$. Referring to figure 11, we have $r = x_2 + y_2$; where $x = r \cos \alpha$, and $y = r \sin \alpha$.

Assuming the magnetizing effect of each set of coils in the transformer to be proportional to the current—which may be admitted for weak degrees of magnetization—then $x = K e$ and $y = K e^1$, where K is a constant and e and e^1 the current in both sets of coils respectively. Supposing, further, the field of the generator to be uniform, we have for constant speed $e^1 = K^1 \sin \alpha$ and $e = K^1 \sin (90^\circ + \alpha) = K^1 \cos \alpha$, where K^1 is a constant. See figure 12.

Therefore,
$$x = K e = K K^1 \cos \alpha ;$$
$$y = K e^1 = K K^1 \sin \alpha, \text{ and}$$
$$K K^1 = r.$$

That is, for a uniform field the disposition of the two coils at right angles will secure the theoretical result, and the intensity of the shifting poles will be constant. But from $r^2 = x^2 + y^2$ it follows that for $y = 0$, $r = x$; it follows that the joint magnetizing effect of both sets of coils should be equal to the effect of one set when at its maximum action. In transformers and in a certain class of motors the fluctuation of the poles is not of great importance, but in another class of these motors it is desirable to obtain the theoretical result.

In applying this principle to the construction of motors, two typical forms of motor have been developed. First, a form having a comparatively small rotary effort at the start but maintaining a perfectly uniform speed at all loads, which motor has been termed synchronous. Second, a form possessing a great rotary effort at the start, the speed being dependent on the load.

These motors may be operated in three different ways: 1. By the alternate currents of the source only. 2. By a combined action of these and of induced currents. 3. By the joint action of alternate and continuous currents.

The simplest form of a synchronous motor is obtained by winding a laminated ring provided with pole projections with four coils, and connecting the same in the manner before indicated. An iron disc having a segment cut away on each side may be used as an armature. Such a motor is shown in figure 9. The disc being arranged to rotate freely within the ring in close proximity to the projections, it is evident that as the poles are shifted it will, owing to its tendency to place itself in such a position as to

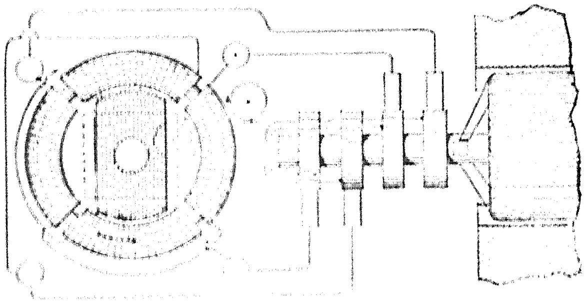


Fig. 9.

embrace the greatest number of the lines of force, closely follow the movement of the poles, and its motion will be synchronous with that of the armature of the generator; that is, in the peculiar disposition shown in figure 9, in which the armature produces by one revolution two current repulses in each of the circuits. It is evident that if, by one revolution of the armature, a greater number of impulses is produced, the speed of the motor will be correspondingly increased. Considering that the attraction exerted upon the disc is greatest when the same is in close proximity to the poles, it follows that such a motor will maintain exactly the same speed at all loads within the limits of its capacity.

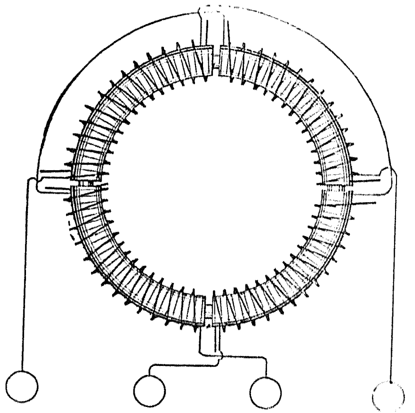


Fig. 10.

To facilitate the starting, the disc may be provided with a coil closed upon itself. The advantage secured by such a coil is evident. On the start the currents set up in the coil strongly energize the disc and increase the attraction exerted upon the same by the ring, and currents being generated in the coil as long as the speed of the armature is inferior to that of the poles, considerable work may be performed by such a motor even if the speed be below normal. The intensity of the poles being constant, no currents will be generated in the coil when the motor is turning at its normal speed.

Instead of closing the coil upon itself, its ends may be connected to two insulated sliding rings, and a continuous current supplied to these from a suitable generator. The proper way to start such a motor is to close the coil upon itself until the normal speed is reached, or nearly so, and then turn on the continuous current. If the disc be very strongly energized by a continuous current the motor may not be able to start, but if it be weakly energized, or generally so that the magnetizing effect of the ring is preponderating it will start and reach the normal speed. Such a motor will maintain absolutely the same speed at all loads. It has also been found that if the motive power of the generator is not excessive, by checking the motor the speed of the generator is diminished in synchronism with that of the motor. It is characteristic of this form of motor that it cannot be reversed by reversing the continuous current through the coil.

The synchronism of these motors may be demonstrated experimentally in a variety of ways. For this purpose it is best to employ a motor consisting of a stationary field magnet and an armature arranged to rotate within the same, as indicated in figure 13. In this case the shifting of the poles of the armature produces a rotation of the latter in the opposite direction. It results therefrom that when the normal speed is reached, the poles of the armature assume fixed positions relatively to the field magnet and the same is magnetized by induction, exhibiting a distinct pole on each of the pole-pieces. If a piece of soft iron is approached to the field magnet it will at the start be attracted with a rapid vibrating motion produced by the reversals of polarity of the magnet, but as the speed of the armature increases the vibrations become less and less frequent and finally entirely cease. Then the iron is weakly but permanently attracted, showing that the synchronism is reached and the field magnet energized by induction.

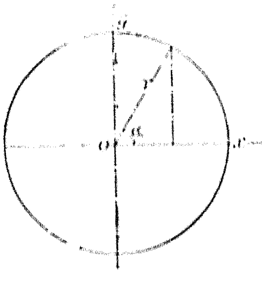


Fig. 11.

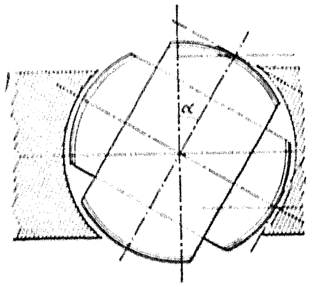


Fig. 12.

The disc may also be used for the experiment. If held quite close to the armature it will turn as long as the speed of rotation of the poles exceeds that of the armature; but when the normal speed is reached or very nearly so, it ceases to rotate and is permanently attracted.

A crude but illustrative experiment is made with an incandescent lamp. Placing the lamp in circuit with the continuous current generator and in series with the magnet coil, rapid fluctuations are observed in the light in consequence of the induced currents set up in the coil at the start; the speed increasing, the fluctuations occur at longer intervals, until they entirely disappear, showing that the motor has attained its normal speed.

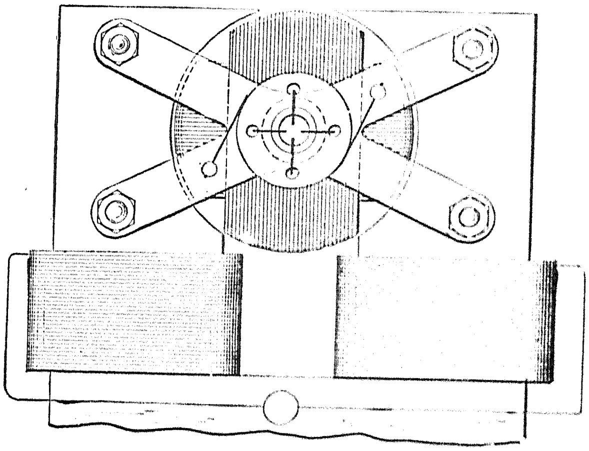


Fig. 13.

A telephone receiver affords a most sensitive instrument; when connected to any circuit in the motor the synchronism may be easily detected on the disappearance of the induced currents.

In motors of the synchronous type it is desirable to maintain the quantity of the shifting magnetism constant, especially if the magnets are not properly subdivided.

To obtain a rotary effort in these motors was the subject of long thought. In order to secure this result it was necessary to make such a disposition that while the poles of one element of the motor are shifted by the alternate currents of the source, the poles produced upon the other elements should always be maintained in the proper relation to the former, irrespective of the speed of the motor. Such a condition exists in a continuous current motor; but in a synchronous motor, such as described, this condition is fulfilled only when the speed is normal.

The object has been attained by placing within the ring a properly subdivided cylindrical iron core wound with several independent coils closed upon themselves. Two coils at right angles as in figure 14, are sufficient, but a greater number may be advantageously employed. It results from this disposition that when the poles of the ring are shifted, currents are generated in the closed armature coils. These currents are the most intense at or near the points of the greatest density of the lines of force, and their effect is to produce poles upon the armature at right angles to those of the ring, at least theoretically so; and since this action is entirely independent of the speed—that is, as far as the location of the poles is concerned—a continuous pull is exerted upon the periphery of the armature. In many respects these motors are similar to the continuous current motors. If load is put on, the speed, and also the resistance of the motor, is diminished and more current is made to pass through the energizing coils, thus increasing the effort. Upon the load being taken off, the counter-electromotive force increases and less current passes through the primary or energizing coils. Without any load the speed is very nearly equal to that of the shifting poles of the field magnet.

It will be found that the rotary effort in these motors fully equals that of the continuous current motors. The effort seems to be greatest when both armature and field magnet are without any projections; but as in such dispositions the field cannot be concentrated, probably the best results will be obtained by leaving pole projections on one of the elements only. Generally, it may be stated that the projections diminish the torque and produce a tendency to synchronism.

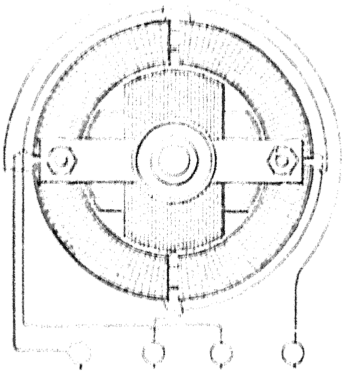


Fig. 14.

A characteristic feature of motors of this kind is their capacity of being very rapidly reversed. This follows from the peculiar action of the motor. Suppose the armature to be rotating and the direction of rotation of the poles to be reversed. The apparatus then represents a dynamo machine, the power to drive this machine being the momentum stored up in the armature and its speed being the sum of the speeds of the armature and the poles.

If we now consider that the power to drive such a dynamo would be very nearly proportional to the third power of the speed, for that reason alone the armature should be quickly reversed. But simultaneously with the reversal another element is brought into action, namely, as the movement of the poles with respect to the armature is reversed, the motor acts like a transformer in which the resistance of the secondary circuit would be abnormally diminished by producing in this circuit an additional electro-motive force. Owing to these causes the reversal is instantaneous.

If it is desirable to secure a constant speed, and at the same time a certain effort at the start, this result may be easily attained in a variety of ways. For instance, two armatures, one for torque and the other for synchronism, may be fastened on the same shaft and any desired preponderance may be given to either one, or an armature may be wound for rotary effort, but a more or less pronounced tendency to synchronism may be given to it by properly constructing the iron core; and in many other ways.

As a means of obtaining the required phase of the currents in both the circuits, the disposition of the two coils at right angles is the simplest, securing the most uniform action; but the phase may be obtained in many other ways, varying with the machine employed. Any of the dynamos at present in use may be easily adapted for this purpose by making connections to proper points of the generating coils. In closed circuit armatures, such as used in the continuous current systems, it is best to make four derivations from equi-distant points or bars of the commutator, and to connect the same to four insulated sliding rings on the shaft. In

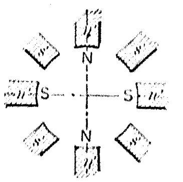


Fig. 15.

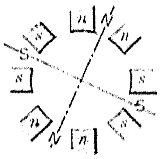


Fig. 16.

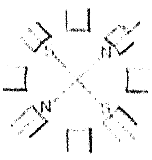


Fig. 17.

this case each of the motor circuits is connected to two diametrically opposite bars of the commutator. In such a disposition the motor may also be operated at half the potential and on the three-wire plan, by connecting the motor circuits in the proper order to three of the contact rings.

In multipolar dynamo machines, such as used in the converter systems, the phase is conveniently obtained by winding upon the armature two series of coils in such a manner that while the coils of one set or series are at their maximum production of current, the coils of the other will be at their neutral position, or nearly so, whereby both sets of coils may be subjected simultaneously or successively to the inducing action of the field magnets.

Generally the circuits in the motor will be similarly disposed, and various arrangements may be made to fulfill the requirements; but the simplest and most practicable is to arrange primary circuits on stationary parts of the motor, thereby obviating, at least in certain forms, the employment of sliding contacts. In such a case the magnet coils are connected alternately in both the circuits; that is, 1, 3, 5.........in one, and 2, 4, 6.........in the other, and the coils of each set of series may be connected all in the same manner, or alternately in opposition; in the latter case a motor with half the number of poles will result, and its action will be correspondingly modified. The figures 15, 16, and 17, show three different phases, the magnet coils in each circuit being connected alternately in opposition. In this case there will be always four poles, as in figures 15 and 17, four pole projections will be neutral, and in figure 16 two adjacent pole projections will have the same polarity. If the coils are connected in the same manner there will be eight alternating poles, as indicated by the letters *n' s'* in figure 15.

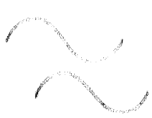


Fig. 18.

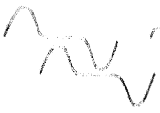


Fig. 19.

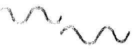


Fig. 20.

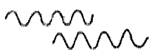


Fig. 21.

The employment of multipolar motors secures in this system an advantage much desired and unattainable in the continuous current system, and that is, that a motor may be made to run exactly at a predetermined speed irrespective of imperfections in construction, of the load, and, within certain limits, of electromotive force and current strength.

In a general distribution system of this kind the following plan should be adopted. At the central station of supply a generator should be provided having a considerable number of poles. The motors operated from this generator should be of the synchronous type, but possessing sufficient rotary effort to insure their starting. With the observance of proper rules of construction it may be admitted that the speed of each motor will be in some inverse proportion to its size, and the number of poles should be chosen accordingly. Still exceptional demands may modify this rule. In view of this, it will be advantageous to provide each motor with a greater number of pole projections or coils, the number being preferably a multiple of two and three. By this means, by simply changing the connections of the coils, the motor may be adapted to any probable demands.

If the number of the poles in the motor is even, the action will be harmonious and the proper result will be obtained; if this is not the case, the best plan to be followed is to make a motor with a double number of poles and connect the same in the manner before indicated, so that half the number of poles result. Suppose, for instance, that the generator has twelve poles, and it would be desired to obtain a speed equal to $\frac{12}{7}$ of the speed of the generator. This would require a motor with seven pole projections or magnets, and such a motor could not be properly connected in the circuits unless fourteen armature coils would be provided, which would necessitate the employment of sliding contacts. To avoid this, the motor should be provided with fourteen magnets and seven connected in each circuit, the magnets in each circuit alternating among themselves. The armature should have fourteen closed coils. The action of the motor will not be quite as perfect as in the case of an even number of poles, but the drawback will not be of a serious nature.

However, the disadvantages resulting from this unsymmetrical form will be reduced in the same proportion as the number of the poles is augmented.

If the generator has, say, n, and the motor n_1 poles, the speed of the motor will be equal to that of the generator multiplied by $\frac{n}{n_1}$.

The speed of the motor will generally be dependent on the number of the poles, but there may be exceptions to this rule. The speed may be modified by the phase of the currents in the circuits or by the character of the current impulses or by intervals between each or between groups of impulses. Some of the possible cases are indicated in the diagrams, figures 18, 19, 20 and 21, which are self-explanatory. Figure 18 represents the condition generally existing, and which secures the best result. In such a case, if the typical form of motor illustrated in figure 9 is employed, one complete wave in each circuit will produce one revolution of the motor. In figure 19 the same result will be effected by one wave in each circuit, the impulses being successive; in figure 20 by four, and in figure 21 by eight waves.

By such means any desired speed may be attained; that is, at least within the limits of practical demands. This system possesses this advantage besides others, resulting from simplicity. At full loads the motors show an efficiency fully equal to that of the continuous current motors. The transformers present an additional advantage in their capability of operating motors. They are capable of similar modifications in construction, and will facilitate the introduction of motors and their adaptation to practical demands. Their efficiency should be higher than that of the present transformers, and I base my assertion on the following:—

In a transformer as constructed at present we produce the currents in the secondary circuit by varying the strength of the primary or exciting currents. If we admit proportionality with respect to the iron core the inductive effect exerted upon the secondary coil will be proportional to the numerical sum of the variations in the strength of the exciting current per unit of time; whence it follows that for a given variation any prolongation of the primary current will result in a proportional loss. In order to obtain rapid variations in the strength of the current, essential to efficient induction, a great number of undulations are employed, from this practice various disadvantages result. These are increased cost and diminished efficiency of the generator, more waste of energy in heating the cores, and also diminished output of the transformer, since the core is not properly utilized, the reversals being too rapid. The inductive effect is also very small in certain phases, as will be apparent from a graphic representation, and there may be periods of inaction, if there are intervals between the succeeding current impulses or waves. In producing a shifting of the poles in a transformer, and thereby inducing currents, the induction is of the ideal character, being always maintained at its maximum action. It is also reasonable to assume that by a shifting of the poles less energy will be wasted than by reversals.

DISCUSSION.

Mr. T. C. Martin:—Prof. Anthony I believe, is here, and as he has given this subject some attention I think he might very properly supplement Mr. Tesla's paper with some remarks.

Mr. Tesla:—I want to express once more my best thanks to Prof. Anthony for aiding me in many respects, and I hope he will be able to enlighten you upon many of the features in this system that I was unable to explain at present.

Prof. W. A. Anthony:—Mr. President and Gentlemen: I have been referred to as having had something to do with these forms of motor. I am very glad to be able to add my testimony to what Mr. Tesla has already given you in regard to their action, and I confess that on first seeing the motors the action seemed to me an exceedingly remarkable one. After my first visit to Mr. Tesla's works some of the motors, I think these very two that you see here on the table, were brought to me to make some tests of their efficiency, and probably you will be more interested in that than anything else that I might say. I am sorry to have not brought with me the exact figures we obtained, but I can give you from memory something of the result. This little motor that you see here gave us about half a horse power, and had an efficiency of something above fifty per cent., which I considered a very fair efficiency for a motor of this size, as we cannot expect with such small motors to get as high efficiency as we can with larger ones. This I believe is the armature that Mr. Tesla calls the armature for high rotary effort. This little pulley which is only about three inches in diameter gave a pull of something like fifty pounds, as I remember it, on turning on the current, so that you see the rotary effort is very considerable and that is also shown in the quickness with which the armature willreverse its motion on reversing the relation of the two currents which pass through the two opposite coils. That could be done by shifting two of the wires or simply shifting a reversing key in one of the circuits, and the armature would stop and reverse its motion so quickly that it was almost impossible to tell when the change took place. That shows, as I have said, the very considerable rotary effort that the armature presents.

This motor (referring to the second specimen) gave us, I think, about one and one quarter horse power and showed a somewhat higher efficiency than the other, a little over sixty per cent. This would run, with the armature as here constructed, almost at the speed of the generator even under a very heavy load. When the load was brought up to the maximum load, where the efficiency began to fall off somewhat, the speed of rotation was reduced. As I remember now, it was reduced to about twenty-eight hundred, and the speed, you see, kept up very nearly to that of the generator under heavy load.

I can add very little to what Mr. Tesla has already given you in regard to these motors. I have no question but that all of you would be as much interested as I was in seeing them work. That is really the best way in which to determine what the motors will do. (Applause.)

Mr. Tesla:—Mr. President and Gentlemen: Prof. Anthony just made the remark that the speed of this motor fell off when the load was increased. That was due to the fact that this armature was designed to secure a considerable effort from the start. But if we make an armature which is only designed for synchronism, the speed will always be the same no matter what the load; only there is a disadvantage that at the outset the rotary effort is so small that it is apt not to start. It would generally start if put in a proper position, but if not put in a proper position it might not start. If we employ an armature consisting of a cutaway block of steel with a coil, it will maintain speed at all loads. The importance of maintaining the intensity of the pole constant is that, if this can be produced, we can utilize, instead of the subdivided armature, an ordinary steel block with the same result. It is only desired to close the magnetic field. You can readily see, if the poles are fixed, that it is not necessary to subdivide the armature, if the intensity of the force is constantly maintained the same. But, if the intensity is not maintained the same, then it is necessary to subdivide, and generally in the results that I have obtained I have found that it is necessary to subdivide. I attribute the good results obtained by the tests of Professor Anthony to the fact that the dynamo used by him has a powerful field and a small armature, and the field is very concentrated, and for that reason, probably, the result is nearer the theoretical.

Prof. Thomson:—I have certainly been very much interested in the description given by Mr. Tesla of his new and admirable little motor. I have, as probably you may be aware, worked in somewhat similar directions and towards the attainment of similar ends. The trials which I have made have been by the use of a single alternating current circuit, not a double alternating circuit; a single circuit supplying a motor constructed to utilize the alternation and produce rotation. I have carried on since the last annual meeting of the Institute the development and perfecting, as far as my time allowed, of a closed circuit armature, if we may so term it, related to the alternating field. That is the plan which I used and which I brought to the notice of the Institute last year: it was to make a laminated field and in that field to place an armature, also laminated, winding upon the armature a coil, which periodically is close-circuited during the revolution by a suitable commutator or circuit closing device. I have made several such motors on different designs and they uniformly start from a state of rest and develop power, and some of them show, at speeds close to the rate of alternations of the dynamo, a tendency to synchronize. Their rotary effort in most cases is a little greater near that point than at other points. I hope before a long while to bring most of these results before the notice of the Institute, and I would therefore prefer delaying further remarks on motors of this description. I certainly think there is a field for alternating motors, and there is undoubtedly an opportunity for obtaining motors which possess even some advantages over the continuous current motors. (Applause.)

Mr. Tesla:—Gentlemen, I wish to say that the testimony of such a man as Prof. Thomson, as being foremost in his profession, flatters me very much. I might say that I have worked in the same line with Prof. Thomson at a period when the invention of Prof. Thomson was not known to me. I had a motor identically the same as that of Prof. Thomson, but I was anticipated by him. I believe that although that particular form of motor has the disadvantage that a pair of brushes must be employed to short-circuit the armature coil, that such a form of motor may be made practicable, for the simple reason that a motor resembles a transformer, and such a transformer, we well know, can be brought to a very high efficiency. On the other hand, the armature may be provided with conductors that are of comparatively low resistance, and it is a mere matter of making a perfect arrangement for short circuiting. You will see the advantage of this disposition of the closed circuit coil—that this action is maintained always at the maximum, and it is indeed more perfect than if the polarities were shifted by means of a commutator.

THE CHAIRMAN:—Unless there are some other remarks on this subject we shall have to pass to the consideration of the next paper. I think thanks are due to Mr. Tesla for his paper. The next paper in order is "Underground Electrical Systems in Europe and America," by Prof. G. W. Plympton, of the Brooklyn Subway Board.

PROF. PLYMPTON:—Mr. President and Gentlemen of the Institute. This is a place in which I should seek to stand for instruction rather than to impart any information. I have looked for a long time to gentlemen of this organization for such information as should guide me in the work I am doing. I shall be very brief with the so-called paper, and will give a little of what I have to communicate, orally in the way of a description of some plates and a narrative of my journey.

Prof. Plympton then read the following paper:

UNDERGROUND ELECTRICAL CONDUCTORS IN EUROPE AND AMERICA.

BY PROF. G. W. PLYMPTON.

The problems of construction of underground systems of electrical conduction will have been solved when the telephone and the arc light systems are both buried under our streets, without impairing the efficiency or durability of either.

By this I mean that all the difficulties encountered in burying conductors are involved in converting telephone and arc light from aerial to underground systems. Telegraph lines and systems of incandescent lighting present fewer difficulties in the process of burying, and none of a kind not met with in dealing with the systems first mentioned.

The telephone problem is substantially solved. Some details only remain to be settled, among which may be mentioned the best size of conductor, the most serviceable insulation, and the maximum distance of effective service for either grounded or metallic circuits.

In Brooklyn, the general plan adopted is that of a conduit divided into ducts, through which cables containing from sixty to one hundred wires are drawn. The material of the conduit is for the most part creosoted wood. About ten miles of this is already in use in our city, and about four and a half miles of the Dorsett concrete conduit. For the extensions of the underground systems for the present year, only creosoted timber is to be used.

Among the lessons learned from our experience are: 1st. That in creosoted conduits the use of cables covered with kerite or any similar rubber or gutta percha compound must be avoided. 2d. That in the so-called lead-covered cables, the use of pure lead is also to be avoided, as it is slowly converted into a porous and friable lead carbonate.

An alloy of lead, with five or six per cent. of tin seems to resist the destructive action. 3d. That the conducting wire first used is too small for satisfactory telephone service. The difficulties of induction and retardation led to complaints as soon as 4,000 or 5,000 feet of underground wire was put in service. The cables that are now being put in are made up of wires whose cross section is greater by one-third than that of the first wire, and they are protected by twice the thickness of insulation.

I mentioned the above conclusions as having been drawn from our own experience in Brooklyn.

In commencing our work we gathered such information as could be gleaned from localities where solutions of the problem had already been attempted. The impression so largely prevailed that in Europe all telephone and telegraph conductors were underground that our Board decided that a personal inspection of European systems should be made by one of our number. The duty devolved upon me. The result of my inquiry has been published in the scientific papers.

In regard to the burial of the arc light wires, I can only say that no method yet tried seems certain of success. Most of them certainly insure the destruction of the underground conductors in from one to three years. But I have no doubt that a solution of the problem will soon be reached. Although the system will be kept apart from the telephone and telegraph subway, it does not seem likely that arc light conductors will be allowed in the same conduit with telephone wires, nor will they be distributed from the same man-holes.

In saying that I believe that a solution of the problem will soon be found, I do not mean to assert that casualties like that recently recorded of a man in the Bowery, who lost his life by grasping the naked wire close to an arc lamp, can be prevented by any system of burying wires. To prevent such *accidents* (if that is the term to be used), the arc lights must be buried with the wires.

All past experience teaches us to proceed cautiously. Nothing can now permanently check the growth of the telephone, the telegraph or the electric light; they have become necessities of our civilization, and any hasty or ill-advised enforcement of the law to convert all aerial to underground systems which should result in serious injury to them, would prove the surest way to perpetuate the nuisance of overhead wires and poles in the streets.

After reading his paper Professor Plympton proceeded as follows:

The impression that I had received from conversations with people who had seemed to know, was that a large part of the telephone service of Europe was buried in the streets. I was partly disabused of that idea by an engineer who had returned from Europe just as I was setting out. I went to London. I was furnished with letters to various engineers, by engineers and managers of telephone and telegraph companies and civil engineers. I first made the acquaintance of Mr. Preece in London, and learned from him that the Postal Telegraph wires were buried in the streets and that some telephone wires were buried with them—enough for the service of the Post Office Department. A telephone company enjoying franchises in London have run all of their wires overhead, as the manager informed me he could not put a wire underground without an act of Parliament.. They bought their right of way over the housetops. There are certainly wires enough in many places there. They do not compare in number and entanglement with ours in New York, simply because there is no such telephone service there, as we are familiar with here, nor is there in any other place. I had submitted the question as to the material of the conduits. He said that after going through a good deal of experience they had adopted iron pipes for their conduits and drew their cables loosely through them. Wires were protected singly, and then two or three, or ten, or a dozen wires were twisted together and pulled through the iron pipe—generally a three inch pipe, rarely larger than four inch. If it was necessary to make connection at some point not contemplated before the pipe was put down they simply knocked a hole in the side of the pipe with a hammer and made the connection. When inquiry was made what they would do in case of so many subscribers along any line of telephone conduit such as we have here in New York, they said that problem was not yet presented to them in London. I examined the system in the streets. These pipes were buried about sixteen or eighteen inches, rarely as much as two feet, ending in little cast iron boxes about twenty inches by sixteen, and perhaps a foot deep, fitted with an iron cover tightly cemented. They sent workmen out, and I was accompanied by Mr. Fleetwood to examine the boxes. They had difficulty in raising one or two. I asked why they were so tightly cemented. They said to keep water out. I asked if that was really the effect. They said they thought it was, but the first cover they took off showed me a box with about two inches of water in the bottom. In submitting the question of material for a conduit to Mr. Preece, he answered that iron was used because it would serve better to protect their wires from injury when the streets were dug up by workmen; pick-axes would knock everything else to pieces. He spoke well of our plan of creosoted timber, and took from a case in his office a piece of an old conduit put down in 1853. It had been in the ground thirty-one years. It came out perfectly sound. The cable had given out years ago. It is known that they began systems of telegraphy in Europe by burying all their wires. They tried to bury them along their lines of railway; afterwards dug them up, and now their wires across country are on poles. They are burying in some places in the cities.

I visited Brussels next, and there found a system of arc lighting. Wires were stretched along the street. None were buried. Telephone wires are over the housetops, the wire being in some cases a hard drawn copper wire, in others a silicon bronze. As the houses are high and the house attachments are high, these wires run pretty well up in the air, and as they are small, they do not attract attention as they do here. That is the case generally throughout Europe. They use a small wire and the housetop fixtures are so high that they are not apt to attract attention. A gentleman on the steamer gravely told me that he had been all over Europe and did not see any wires in the air at all; he knew they were all underground. As the chief manager of the telephone system was in Antwerp, I went on to Antwerp to get information about such telephone service as was under his control. That was Mr. DeGroot. He very kindly offered his time to answer as many questions as I chose to ask. I thought of going on to Stockholm for the reason that I understood the service was very good there, but I learned from Mr. DeGroot that everything was put up under his direction, and with the exception of the frame work on the house-tops everything in Stockholm was duplicated in Antwerp. He thought that no telephone wires in Europe were underground except those that were in the sewers in Paris. He proved to be not quite correct, though pretty nearly so. I asked about his housetop connections. He said all through the Belgian and Scandinavian cities the wires were on house-top fixtures. In Italy they were on brackets under the eaves, for the reason that the tiling of the roof does not support very kindly these immense wooden structures. To preserve some degree of coolness in their roofs they put on a triple tiling with little air spaces between the tiles, and it almost destroys the whole roof to put up a wooden structure. Hence, they attach the frame by brackets under the eaves. The style of these you will have an opportunity to see, as I have had these systems extensively photographed in many places in Europe. Mr. DeGroot added that one company had come there with an electric lighting system—and had buried a cable for a short distance, with the intention of lighting the street. It was all buried and they were nearly ready to start, when the gas companies came forward and proved that they alone had the right to dig up the street—the right of *canalization* of the streets, as he expressed it. The whole plant was lost and now lies buried in the street. He mentioned as an interesting fact that he tried a new plan along the shore of the Scheldt. He had just put up a mile of poles to support his wire. That was a pole line in Europe that they felt rather proud of. I had it photographed and received the photograph since. In Berlin the circumstances are not widely different. There is a series of lines under the municipal direction and some under the direction of the imperial government. There has been a little friction between the two. The first official that I consulted assured me that all telephone wires were on the house-tops; nearly all telegraph wires were buried in the streets and at no great depth, very much like the wires in London, in pipes, not far below the surface. The German military telegraph is of another kind. That is the Siemens cable strongly wound with an armor and buried without any other protection directly in the ground. The telephone wires are very abundant in Berlin over the house-tops. There is quite a tangle of them as you will see by many photographs here. I should add that in Brussels perhaps the most remarkable display of wires anywhere is shown by the photograph, which is a fine picture of their chief cathedral, with wires spread above and running a long distance to their support. In Berlin the house-top fixtures are really very extensive. I never have seen any in New York which were able to carry as many wires at once as those of Berlin. As the government controls the placing of the wires, they are frequently placed on their best buildings and some of the very fine views across their favorite avenues show these light iron fixtures on top of public buildings. The difference between the German service and that of Belgium, is that in Germany the householder must submit to having the fixtures put on his house. He is paid, however, for the service. In Belgium they make the best terms they can, and the telephone companies pay at about the rate of ten cents per wire per year for the use of the house-top.

One picture represents the central station in Turin where there is a great assemblage of wires coming over the house-tops. In Brussels one difficulty which Mr. DeGroot said he had experienced was from the disturbance of the wires by angry citizens. The wires were so thick that they killed a great many carrier pigeons. Owners of pigeons would get angry and go up on the house-tops and cut down the wires. Fifty wires were destroyed the day before. An old lady who owned a number of pigeons had cut them.

Cologne has, I think, about five hundred telephones—it was August, 1886, that I was there—and Frankfort about four hundred. Four, five or six hundred is about the number for the cities in central Europe.

In Paris a different condition prevailed. I went directly to the electrician. I had letters from Mr. Edison and Mr. Vail introducing me to M. Berthon, in whose charge are all the telephone lines of France. I asked him immediately about his underground system in the sewers. He said, "Yes, they are in the sewers, but they are there because the sewers are right at hand." I asked him, "What do you do when you come to the end of your sewers?" He said, "Then we come out and go to the house-tops." A little later he showed me a wire drawn out of a sewer—drawn right up, fastened to the side and drawn up to the house where it is wanted. I asked him what was the plan in other French cities. He said they were all on the house-tops except in Bordeaux, where about three hundred subscribers are furnished by a system drawn through iron pipes, but the system is not doing very well. I visited the sewers and walked along through them some distance. I noticed the method of attaching. It was a little hook with a shank, driven into the wall over head; I could just reach it with my hand. That would hold four or five cables each as large as my thumb. There would be twelve or fourteen of those small cables in a single one of these sewers. It was easy to make out the distance of these cables as they branched off, because the sewers are very conveniently marked; the street corners are marked with the name of the streets. In regard to the arc lighting, I asked him if he would put the arc light wires in the same sewers with his telephone wires, and he said "No," very energetically—he would not have them there; indeed, he did not believe that arc lighting was going to prevail to any great extent, and then added, "We have less now in this avenue that you are in (Avenue de l'Opera) than we had three or four years ago; we have replaced the arc lights with the gas lights." The gas lights are in groups, five or six burners in each lamp. There were no arc light wires underground, so far as he knew. I noticed while walking with him along the street that there were some wires overhead. He said these were some of the dead wires not yet removed; but he called the superintendent, who was in sight, and submitted the question to him. The superintendent said no, that the wires were overhead, as there was no sewer in that street; it was a short street. Then M. Berthon added, "There are one hundred subscribers served that way," and again the superintendent interposed to say that there were two hundred in Paris served over house-tops solely. M. Berthon added that in regard to the telephone service that he expected to be able to extend it through Spanish cities; it was a franchise that he was expecting to control very soon, but he had made it a condition that he should be allowed to go on house-tops. So I got but little information with regard to the best way of burying wires until I got back to England, when, after a short stay and another consultation with the electricians there, I went on to Newcastle. I was assured that they were beginning to put wires underground in Manchester. If I went there I should see that they were burying a cable in an iron pipe. Newcastle enjoys the reputation of having all its wires underground. They have a system of iron pipes. They serve some three hundred or four hundred telephone subscribers by the use of the wires controlled by the Government. They do not study economy in the method of hauling in wires or in the use of them. They twist their wires together so as to to establish a metallic circuit and run from their central station to their customers.

I had got this estimate from both M. Berthon and Mr. Preece:—that when the underground system was fully at work with cables, the best that they knew how to make, the limit of service would be about ten or eleven miles underground. I asked if it would be profitable to run further than that if the conductors came out of the ground at that distance. They said that was doubtful; it was a question not solved. M. Berthon gave twelve miles as about the limit which it was possible to send a telephone message through anything buried in the ground. I mentioned that to the electrician who was assigned to me by Mr. Heaviside, the head of the telephone service in Newcastle, and he said, "I cannot quite accept that estimate, and I think I will give you an opportunity to refute it." So he connected up a line for me to experiment with, which he said was a line of twelve and a half miles of cable—what he called cable, that is, a lot of wires twisted together in pairs, twisted a little way in one direction and then twisted in the opposite direction. These pairs were put together until there would be about a dozen or twenty and then they were pulled loosely through the iron pipe. At the end of the twelve and a half miles the wire came out of the ground and connected for about thirty-eight miles further. It was fifty miles and a fraction through which he said the telephone service extended. I could converse through that, though I found difficulty, where an expert worker at the telephone would not. I could converse through that when the conditions were exceptionally favorable. It was in a padded room and the instrument was in perfect working condition. I had no difficulty in conversing through that twelve and a half miles of cable and the aerial line up to fifty miles. Such telephone service as they have in Newcastle would hardly answer the purpose where there are so many users as we have here. I asked how they increased the number if they had any additional subscribers—how could they pull more wires into the iron pipe that had a twisted tangle of wires already in it. He said they pulled the whole bundle out and sent them to headquarters—the Postal Department—and they furnished them a new lot. Economy was not considered. He said the Government owned the whole thing.

DISCUSSION.

MR. J. A. POWERS: As a central station operator, I should be glad to ask if there are any arc light wires buried underground; what is the longest time for which such wires have been buried up to the present time?

PROF. PLYMPTON: I cannot tell that. We have had no experience of that kind in Brooklyn. The arc light company appears to be ready to try experiments when they see their way clearly. We have been more interested in getting the telephone wires underground, because they outnumber all the others.

MR. POWERS: There seems to be in this matter a very grave misunderstanding by the public about the danger of the electric light wires themselves. I have kept a close track of this subject since there has been some agitation of this underground question at Troy. I have also kept close track of the accidents recently, and I have noticed that four out of five do not occur from the electric light wires directly, but from the coming down on the electric wires of, I think, principally, abandoned wires overhead. The nuisance has become so great in Troy that a law was recently passed by the Common Council requiring the city engineer to remove those wires, and there is now a mutual action of the telephone, telegraph and electric light companies to see to the removal of those wires. We had one accident a short time since. An abandoned Atlantic and Pacific telegraph wire fell down across ours and killed a horse, through no prime fault of the electric light company. If we can get the telegraph and telephone wires underground, which seems to be possible, we shall have, if the recent accidents are any criterion by which to judge, four-fifths of the danger removed at once, and I think that the question of danger will then sink into comparative insignificance.

MR. C. O. MAILLOUX: I would like to ask Prof. Plympton if he has any data regarding the working of the underground system at Chicago. I believe the underground system has been working there on a considerable scale. It might be interesting for him to tell us whether he has any personal knowledge regarding it. His treatment of the underground systems in Europe is certainly a very masterly, and thorough and comprehensive one, and there is but very little for us to say either by way of comment or otherwise. But it is quite evident to the minds of us all, that they have not done anything in Europe that we can safely and conscientiously recommend for our own particular cases, because not only are the systems more numerous in this country, but they are also exploited on a much larger scale, so that a remedy which might be adequate in Europe would not be adequate here, and for that reason I would like to know what Prof. Plympton may have learned of systems which are promising success in this country.

Prof. Plympton:—I visited Chicago and was very politely received by Mr. Barrett, who is superintendent of the work there. He said that he began by forming an agreement with various companies that they should bury their wires as fast as he buried the wires of the city. He is City Electrician. He allowed the companies to select their own conduits, so that each company has its own. They have telephone wires, telegraph wires and underground electric light wires for some purposes. There is no municipal street lighting in Chicago, or was not a year ago last February. Some theatres and buildings are lighted, but the wires run through conduits. That was the case where one of the conduits took fire. I wrote to Mr. Fay and asked about the telephone service, to learn if they had suffered from induction owing to proximity to any of these other wires. They had in some cases. The harmonic system of telegraphy had created some difficulty through its induction, and there had been complaints, but inasmuch as each company had selected its own conduit, and buried its wires in its own way, there was not much to gather about a general system of burying, of getting them into as compact shape as could be done. Indeed, standing at one of the street corners one of the electricians pointed out to me fifteen man-hole covers in sight at the junction of two streets, all within a radius of less than sixty feet. They have there a short way of making connection. Their soil is soft, light sand. They take a sharp-pointed pipe and drive that pipe right across the street, if they want to make a house connection. It may turn out in the middle of the track, and then they pull the pipe out and start again.

In regard to getting information from other sources, the remark made by Mr. Parrish in London was significant. He said: "If I had been asked about the best place to go to learn about underground systems, I should have said the United States. We have none here." I found that to be the case. They can come here to learn about underground systems. They cannot overtake our rapid development.

The Chairman: Unless there are some other remarks we will pass to the consideration of the next paper—"The Patent Court and Uniformity in Patent Practice," by Mr. George H. Stockbridge.

Mr. Stockbridge: You will remember that some time ago Mr. Steuart, representing the National Electric Light Association, read a paper before you, in which he pointed out certain reforms that were regarded as necessary by that association, and there was, I believe, a discussion here of the reforms that were pointed out at that time. I believe it was the design of the association to present bills to Congress embodying the reforms suggested by Mr. Steuart, and embodying them in a revision of the Patent law. That scheme, however, met with opposition from some, and instead of that, two bills were presented by the association to Congress, and one of them provides that a patent court be established to have appellate jurisdiction from the Commissioner of Patents in all rejected cases, and the other is that the Commissioner of Patents receive a salary of $8,000. Mr. Steuart asked me to prepare an argument to be submitted to the Judiciary Committee, before whom the bill on the patent court now is, and I prepared such an argument, taking rather a narrow point of view, so as not to interfere with different arguments that were to be made by the other parties.

Mr. Stockbridge then read the following paper.

THE PATENT COURT AND UNIFORMITY IN PATENT OFFICE PRACTICE.

BY GEORGE H. STOCKBRIDGE.

In December, 1885, Mr. Montgomery, then Commissioner of Patents, rendered a decision in the case of *Young* in which he held that a process and product, or any two "kindred and auxiliary" inventions might be claimed in the same application. This decision was a reversal of his predecessor's judgment in *ex parte Blythe* (C. D. 1885, page 82). Whereas, Mr. Butterworth had held that by employing the language, "art, machine, manufacture, or composition of matter," Congress had defined in the law itself certain distinct classes of invention, Mr. Montgomery denies this, in effect, and holds as follows: "Inventions may be distinct and not separate and independent; even if so distinct, if they depend upon each other and mutually contribute to produce a single result, if they are 'kindred and auxiliary,' if they are in their nature or operation connected together, if they are connected in their design and operation, they may be claimed in one patent."

The office practice under Butterworth had been to deny the right of inventors to claim a process and a product, or a machine and a product, or a machine and a process (for these groups have always stood on the same footing) in the same application. The *Blythe* decision did not go so far as to warrant this in direct language, but it was given that scope by the office.

Now, when the Montgomery decision in *Young's* case was published, all this was changed. This decision also was interpreted somewhat differently from what its language would justify, but it led to the practice of combining in a single application duplicate sets of claims on the lines indicated. This practice continued nearly two years. During that time applicants and their attorneys filed, we will say, a thousand cases having claims based upon the *Young* decision. This was done with the clearest warrant, which was the office interpretation of *ex parte Young*.

On Oct. 19, 1887, Mr. Hall, the present Commissioner of Patents, took up the same subject and rendered a decision *ex parte Herr* (41 O. G., page 463), in which he says: "It must be regarded as settled by the very highest authority that an apparatus and a process are separate and distinct inventions. There is no requirement of law that they must or ought to be comprehended in a single patent, while there are many reasons why they ought not to be. A claim for a machine or apparatus and a claim for a process should be prosecuted in separate applications, and each, when allowed, should be comprehended in a single patent." There was no mistaking the meaning of this language. The result of it was that attorneys were notified, in the thousand cases I have mentioned, that the claims, or some of them, which they had filed in good faith and in accordance with the practice of the office, must be stricken out.

A secondary result was that inventors began to ask what their attorneys meant. "You told us," they said, "that we could cover both sets of claims by a single patent. What do you mean, now? Is this an attempt to squeeze us for another fee?" Well, of course, we had to explain. But the inventors then asked, "What does the *Office* mean? Is it possible that after fifty years no settled practice on this point has become established?" We had to answer, No. And in answering my own clients, I was led to consider just how safely such a statement might be made, and the result was the present paper.

Mr. Commissioner Hall expresses it in this way: "I approach the consideration of such a question with the greatest hesitation. It is one that has kept the attention of the office from its foundation up to the present day, and during the entire period there have been constant changes and variations of opinion on the part of commissioners, contrariety of decisions and diversified rules."

I have not seen fit to go back farther than eighteen years, during which time there have been eleven commissioners of patents, and four or five acting-commissioners during the interegna. In 1870 Mr. Commissioner Fisher made a decision in case of *William Lowe* (C. D. 1870, page 39). In this case the inventor, who had been allowed claims for an apparatus, sought to obtain a second patent upon the process which was conducted through his apparatus. In deciding the case on appeal, Mr. Commissioner Fisher said: "The first answer to this claim is, that it cannot form the subject of a separate patent. It is not a distinct and separate part of the thing patented in the original. The furthest point to which the division of inventions has been pushed has been to grant one patent for the process and another for the product of that process. Such patents have, with some hesitation, been sustained (*Goodyear vs. Honsinger*, 3 Fisher, 150; *Goodyear vs. Wait*, 3 Fisher, 244; *Goodyear vs. Providence Rubber Company*, 2 Fisher, 510), but upon the express ground that this was a matter confided by law to the discretion of the commissioner. But I have never known one patent to issue for a machine and another for the law or principle by which it operates. The mechanism of applicant is of no possible value except to carry out this process. Aside from this it possesses no patentable utility. The process is of no avail unless carried on through the medium of instrumentality. (*Morton vs. New York Eye Infirmary*, 2 Fisher, 324.) It would be idle to sell the patent for the mechanism and withhold that which covers its legitimate and only mode of operation. Separate patents for processes and products have been justified upon the ground that the product might be imported, and a patent for the process would give no protection; but the machine and its mode of operation cannot be separated."

Mr. Fisher was a man of very positive opinions, and he leaves no doubt regarding his views on the subject. But the views of his immediate successor, Mr. Leggett, are very different from his. (See *Murray vs. Wuterich*, C. D. 1873, page 96). He draws diametrically opposite conclusions from Judge Clifford's decision in *Goodyear vs. Providence Rubber Company*. Admitting that the office practice allowed a "machine and its product to be claimed in the same application and go out in the same patent," he sums up as follows: "Notwithstanding the precedents w exist, and admitting that such a practice as they indicate, if strictly correct, is not fatal to the validity of patents, I thi time has come when, leaving out of consideration the pers of the patents themselves, and the revenue to be derived the law for examinations, a proper classification in the of as to facilitate examinations and prevent mistakes in the is patents, requires that processes, machines and their produ presented for patent in separate applications."

Mr. Leggett resigned his office in October, 1874, and on the 23d of that month Acting Commissioner Thatcher, in the case of *Chamberlin* (C. D., 1874, page 111) says: "All that can rightfully be claimed in this application is the machine and the link blank, which is the product of the machine." Here is plain evidence of difference of opinion. Mr. Thatcher evidently knows nothing about "a proper classification in the office, requiring that processes, machines and their products be presented for patent in separate applications."

I find no positive judgment of Thatcher on the subject, either while Acting Commissioner or later, when Commissioner, nor of his successor, Mr. Duell, although Duell deals with a case in which process and product claims are combined, (*Loeser*, C. D., 1876, page 104), without adverse comment.

The next decision is that of Doolittle, Acting Commissioner in 1877, in which he demands a division of the application of *Junker* and *Wolf*, on the ground that "the article is not necessarily the result or product of the action of the machine." This is in some sense a forerunner of Mr. Commissioner Spear's decision in 1878, which introduced a mode of treating such cases that prevailed for several years after him. His language is: "No rule of universal application can be given for this class of cases, more than for any other; but in each case, as it comes up for consideration, the inquiry must be made whether the process or machine and the products thereof are so separate and distinct that they must be called independent inventions."

This doctrine was affirmed by Mr. Paine in various decisions, notably in *Winterlich and others*, where he uses this language: "If the machine and the manufacture are so related that the former cannot operate without producing the latter and the latter can only be produced by the former, both may be united in one patent; but this is an exception to the general rule, which forbids the joinder of the machine and its product in one application.

Mr. Commissioner Marble, in *Bancroft* and *Thorn* (29 O. G., page 1,894), applauds the doctrine of his immediate predecessor and lets each case stand upon its own merits. Then came the Butterworth decision to which I have already alluded. He regards art, machine, manufacture and composition of matter as "statutory classes of invention." "Suppose," he adds, "there be invented something in an art, or, as it is ordinarily called, a process, also a machine for carrying out the process, and also the manufacture or article which is produced in the operation of the process by the machine. There can be no doubt but that the statute calls for three patents for these three things. It makes no difference if they are invented simultaneously or successively, by one man or by three. Their independence remains the same."

Now let us see what changes of practice have been introduced by the various decisions to which reference has been made.

Mr. Fisher's decision seems to have been a new departure and to have altered the previous practice of the office. At all events, it fixed the practice under his administration and made it a positive requirement to combine claims for a machine and its product in the same application.

Well, what happens next? Mr. Leggett demands that such claim shall be separated, to facilitate the work of examination. That is change No. 2.

Mr. Thatcher and Mr. Duell appear to have allowed the two sets of claims to be combined, though I find no decision of theirs directly on the point. If I judge rightly of their practice, it may be regarded as constituting change No. 3.

It is certain that Mr. Spear and his successors, Mr. Paine and Mr. Marble, followed the practice of settling each case according to its own circumstances. Call this change No. 4.

Mr. Butterworth returned, in effect, to the practice of Mr. Leggett (change No. 5); Mr. Montgomery, in effect, followed Fisher (change No. 6), and Mr. Hall has now brought back the Leggett decision, thus introducing again a different practice from that of his predecessor in office (change No. 7).

I believe that if each Commissioner had made himself the author of a new practice, there would have been three more changes than have actually taken place. However, I do not think more could have been expected. Really, this is very serious matter. The amount of annoyance and hardship imposed upon inventors in the way of unnecessary delay and expense is in the aggregate very great. I have no disposition to express an opinion as to which one, or whether any one, of the Commissioners has been right in his judgment on the matter in question. I have no doubt that the decisions in every case have been drawn up with a conscientious regard to the responsibility of the office from which the decisions emanated. But I do think that inventors and all who are interested in inventions, have a right to protest against the continuance of a system which admits of and invites such unjust and arduous practices. It would not matter so much what the practice was, provided it were uniform, although of course it ought to be right and fair to inventors. But without uniformity it is monstrously unjust.

Lest it be thought that I have selected a peculiar class of cases, I may hasten to deny it in advance, and may add that the office practice has been frequently altered on many matters quite as important. Take, for example, the question whether more than a single claim may be made in an application for a patent on a composition of matter. At least two commissioners have answered no, and at least two more have answered yes. And so with many other cases. The mere fact that appeals have been taken to successive Commissioners on the same subject indicates the condition of affairs in the office.

What is the remedy for this evident evil? The best remedy yet proposed I conceive to be embodied in the bills recently laid before Congress by the Legal Committee of the National Electric Light Association. One of these bills provides that the salary of the Commissioners of Patents shall be $8,000 a year. I am not sure that the enactment of such a bill would not do away with two-thirds of the evils of the present state of affairs. It is probable at least that we should not have eleven Commissioners of Patents in eighteen years, if the salary were $8,000, and with less frequent changes in the head of the office, there would be necessarily less frequent changes in the office practice.

But there is still greater promise in the other bill, which provides for the establishment of a Court of Patent Appeals, which shall have appellate jurisdiction from the Commissioner of Patents "in all cases touching patentability of inventions, priority of invention among several claimants for patent on the same invention, and the *judicial practice of the Patent Office.*"

Such a court would be in the nature of the case a continuing body, and its decisions would be little liable to change. The enactment of the last clause quoted would give it the proper jurisdiction, and set up what has never been set up before in connection with the Patent Office—a controlling and continuing authority, governing the actions of the Commissioner, and, through him, of the primary examiners.

THE CHAIRMAN: The next paper will be by Prof. George S. Moler on "The Swinging Arm Galvanometer," and in his absence will be read by Professor Nichols.

A SWINGING ARM GALVANOMETER.

BY GEORGE S. MOLER, A. B., B. M. E., CORNELL UNIVERSITY.

The objects sought in the construction of this galvanometer were applicability to a great range of currents, accuracy of indication and portability. In passing from the measurement of large to that of small currents, no change in the apparatus is necessary, and still throughout a wide range great sensitiveness is maintained.

These ends are attained by making use of the fact that the influence of a current on the magnetic needle varies with change of distance and with the angle which the plane of the coil makes with the horizon. The principles of the sliding coil galvanometer and of Obach's cosine galvanometer are thus combined A movable arm is hinged to the base of the galvanometer, Fig. 1,

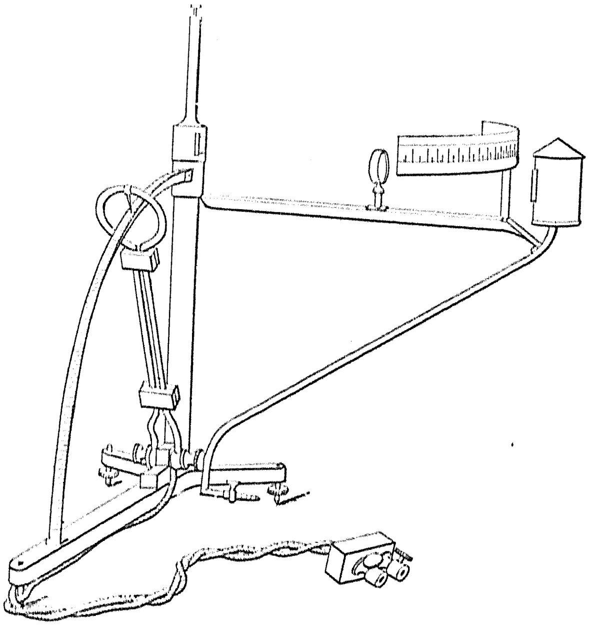


Fig. 1.

and carries the coil, which is attached to its outer end. The length of the arm is such that in the vertical plane the needle will be in the axis of the coil. By means of screw clamps, the hinges are made to produce friction enough to hold the arm in any required position.

When the coil is brought up into the vertical plane, it is very near the needle and a small current will produce a deflection. In this position, which is that of maximum sensitiveness—(since the distance is a minimum and the angle which the coil makes with the horizontal plane is 90°)—$\frac{1}{100}$th of an ampere produces a deflection of about 8 mm.

As the arm is carried down toward the horizontal position, greater and greater currents must be employed to produce the same effect. The position of the coil can be read upon a graduated quadrant which passes through it, there being a pointer attached to the coil and extending inward to the graduations.

The arm consists of two parallel bars of copper, terminating in the two hinges. These are insulated from each other, and from them two flexible conductors are carried to the extremity of the base; after that they are twisted together for a distance of about one meter, and end in a block to which two large binding-posts are attached.

The coil is made of a copper rod 8.5 mm. thick, and its outside diameter is 76.5 mm. The radius of the graduated quadrant is 343 mm. and it is divided into twentieths of an inch. The radius of the lamp-scale is 381 mm. The scale forms the arc of a circle, having the galvanometer mirror for the centre. Its length is 500 mm., and it is composed of paper rendered semi-transparent by being treated with castor oil.

The needle is shown in Fig. 2. It is a bell-shaped magnet carrying a mirror, which can be rotated slightly upon the suspension rod for the sake of adjustment.

The needle is suspended by means of a fine silk fibre, and is prevented from making a complete revolution by means of a stop. Its motions are dampened in the usual manner by the action of a surrounding cylinder of copper.

The rays of light from a small gas lamp, shown in Fig. 1, after passing through a lens, are reflected from the mirror and then fall upon the scale. When the plane of the swinging coil is in the magnetic meridian, and no current is flowing, the spot of light will fall at the centre of the scale. The lamp and scale are attached rigidly to the body of the instrument for the sake of rigidity, and to avoid the necessity of repeated calibrations. In the lamp-case is a small circular opening having a fine vertical cross-hair of platinum, and the lens is so placed that the image of the cross-hair is sharply defined upon the scale. All parts of the scale being equally distant from the mirror, as shown in Fig. 1, the definition of this image is equally good for all deflections.

The swinging-arm galvanometer is especially designed to be used with the method of constant deflections. By carrying the coil up from the horizontal position, the spot of light will be deflected more and more, until the image of the cross-hair is made to coincide with the chosen mark upon the scale; the position of the coil is then to be read and the current found by reference to a calibration curve or chart. In the measurement of feeble currents the chosen point on the lamp-scale may be near the position of no deflection, while for stronger currents it may be further

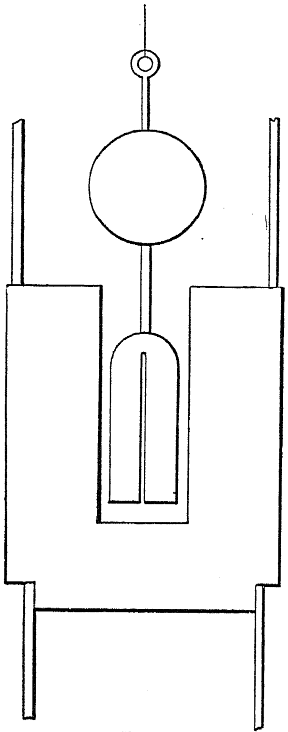


Fig. 2.

From this curve the currents for intermediate positions of the coil may be determined and the degree of accuracy of which the instrument is capable may be estimated.

The Chairman read the communication from the Secretary of the Electric Club, which had been presented at the annual meeting on the previous day, inviting members of the Institute to visit the house of the Electric Club.

THE CHAIRMAN:—I have another announcement to make in regard to Mr. Tesla's motor. I believe that this motor—Mr. Tesla can correct me if I am not right—is the first good alternating current motor that has been put before the public anywhere—is that not so, Mr. Tesla? And he says that the system can be seen in practical operation at 89 Liberty street, second floor, and he invites you all to come and see it. (Applause.)

The next meeting of the Institute will take place on June 12th, and Mr. F. J. Sprague will read a paper on "The Engineering Problems of Electric Railway Work," and I hope that all will be present so as to lend the scientific atmosphere that is needed for papers of this class, to the occasion. (This meeting was subsequently postponed to June 19th.)

The general meeting then adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. V. New York, August, 1888. No. 11.

The twenty-seventh meeting of the American Institute of Electrical Engineers, was held in the Lecture Room, Library Building, Columbia College, Madison Avenue and 49th Street, New York, Tuesday, June 19th, 1888.

President Weston called the meeting to order at 8 o'clock, P. M.

The President:—Ladies and Gentlemen, and members of the Institute: We have with us to-night Mr. Frank J. Sprague, who will read a paper, entitled: "The Solution of the Municipal Rapid Transit Problem," or "Electricity and its Application to Electric Railroads." The matter is fraught with great interest, and as the system of electric railroads has about trebled inside of the last eleven months, it will undoubtedly be very interesting to hear of the work that Mr. Sprague has done in this direction. Few people who have not been engaged in working out experimentally the practical application of electrical appliances on a large scale, can appreciate the amount of labor, patience and perseverance required to bring them to such a state of usefulness as we see the electric railroad has reached. It not only requires that, but a great deal of money; both must be united to produce the desired results. I am sure we shall be pleased to hear what Mr. Sprague has to say, as I understand he has been very successful in overcoming many serious obstacles.

Mr. Sprague was received with applause and read the following paper:

THE SOLUTION OF MUNICIPAL RAPID TRANSIT.

BY FRANK J. SPRAGUE.

The presentation of this subject before the American Institute of Electrical Engineers for discussion seems to me most opportune. It is a question of great importance, and one for which a definite solution is fast becoming imperative. To those of us who believe that the solution is to be found in the application of electricity, it has a special significance, and I trust that what I say may bear fruit, by starting a general discussion of the subject, not only among electrical men but the general public as well.

More attention is now being paid to the question of tramway traffic than ever before. The reason is not only the comparatively high cost of operation under the present almost universal system of horses, but also because the public are beginning to feel, and the street railway men are beginning to see that it is a system behind the times. Laying all humanitarian questions aside the public demand a better and a more rapid service. Railway men want to give it, provided they can do so for the same or less money than they are now spending.

To quote from a New York evening paper:

"The rapid transit question has steadily pressed its way to the front of subjects demanding immediate attention, until it now stands in this city the foremost question of the hour.

"The fact is made evident, by the eager discussion upon every hand, of the very best means by which the end may be attained with the least possible delay. The total passenger traffic, that is, the total number of persons carried in this city, has increased at the rate of over 140 per cent. in each period of ten years since 1866, and is now something over 325,000,000. At the same rate of increase it would amount in 1890 to over 500,000,000, and in 1900 to 1,225,000,000."

There is every reason to believe that the through traffic will increase as much as expected, because the city is growing rapidly and its business section is continually enlarging and pushing northward, forcing the larger part of the population, especially the laboring classes, to seek homes in the upper portion of Manhattan Island and the annexed district, where rents, already increasing because of the increased facility of access, are still lower than can be readily commanded for business purposes in the lower part of the city. The elevated roads of this city, although carrying 500,000 people a day, do not answer the requirements of rapid transit, either local or through; nor is it possible for any two-track railway to do so.

By rapid transit we do not mean merely through transit; we ought not to consider alone the methods of transportation from one end of the city to the other, or from the city to the suburbs; we are likewise called upon to find some means by which the local traffic shall be benefited. Everything which lessens cost of operation, increases speed, reduces intervals between cars, increases the number of cars and makes extensions possible, aids rapid transit, no matter whether surface, underground or overhead.

With the same cost of operation, provided the motive power is materially cheapened, a greater number of cars can be run, intervals can be shortened up, large cars can be run in the place of small ones, better attention can be afforded, cars can be better heated, lighted and cleaned; they can make better time, people can be carried further for the same money, branches can be opened in districts which would otherwise not pay, and what is all important, more passengers can be carried, and for this reason alone more money can be made. For years an attempt has been made to replace the horse with motors of various descriptions—compressed air engines, fireless engines, hot water and soda motors, naptha and petroleum engines and steam engines. But we can safely say that the question has to-day narrowed itself down as between the cable and the various electric systems for surface work, and between electricity and steam for elevated or underground work.

In order that we may be in a better position to judge of the comparative economy and the relative advantage of present and proposed methods of traction, I will briefly consider, first, the use of horses.

Two distinct methods are recognized among street car men in the handling of their stable equipments. In one, the stock of horses is kept as low as possible; they are worked hard, making fourteen or fifteen miles a day, and the depreciation is very heavy. In the other the stable equipment is increased; the horses are kept in excellent condition; their average daily duty is reduced to ten or twelve miles, and the depreciation is lessened.

Assuming the cost of a horse as $150, and the cost of feeding and caring for him as $180 per annum, it would seem that any accurate knowledge of the average life of horses under different day's duty would soon determine the proper amount of the latter.

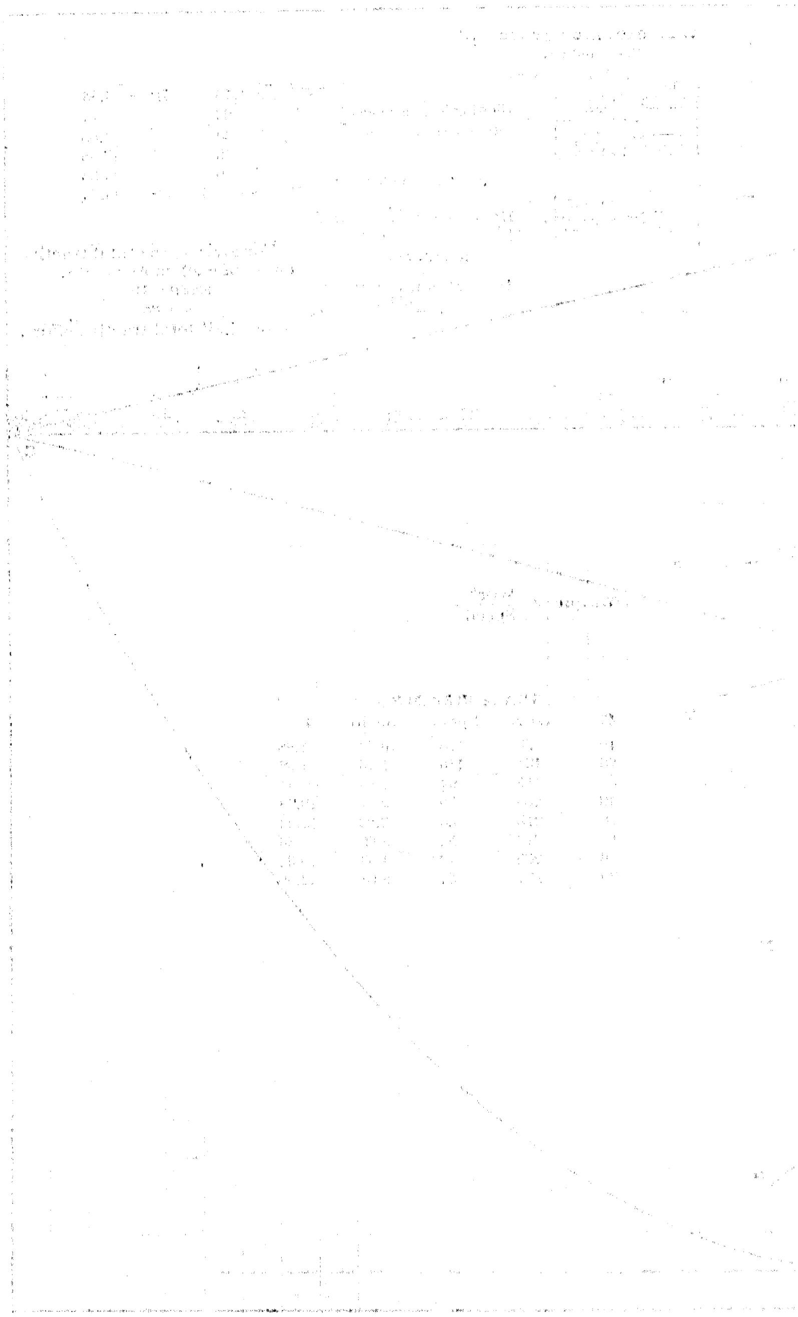
All railway returns do not, strange as it may seem, give accurate information on which such estimates of cost can be made; but from such facts as are given we may deduce the following: Taking the returns for 1887, for the five largest roads in Massachusetts, we find that they show 6,909 horses and 1,410 cars; that the mileage made was 12,834,665, and the passengers carried 76,187,842.

Dividing the average daily mileage by the number of horses, and multiplying the number of horses in a team, we find only 10.26 miles as the average daily duty for all teams, sick or well. Horses which are on duty make from 12 to 14 miles.

On the West End road of Boston, which is now the largest street railway combination in the world, having about 212 miles of track and over 8,000 horses, 10 per cent. of the horses are counted as being off duty from illness, sprains, shoeing or other causes; and the balance of the horses average about 12 miles a day. A car day is estimated at 10 to 11 hours, and from 45 to 50 miles, and eight horses are allowed as the active force.

The rule given me by one of the officials of this company, as a fair one to determine a stable equipment is, to divide the total daily mileage by the miles made in a car day and multiply it by nine, adding what is necessary for hill horses. Hence, if fifty miles is a car day duty, a daily run of 1,600 miles would require a stable equipment of 288 horses, besides those for hill work.

On the Fourth Avenue line in New York, the stable equipment is determined as follows: A car day is 11 hours, and eight horses make about five trips, aggregating about 50 miles. To this number is added 10 per cent. for illness, sprains, etc., and 10 per cent. for emergencies. On this road is illustrated the influence of an important factor in horse car work—that is, the position of the



stables. Horses from the upper stable are limited to 11 miles, or otherwise they would have to make 22 miles, while horses from the lower stable have to make about 13½ miles, or considerably more.

The average cost of motive power per car day throughout the United States, that is, for from ten to eleven hours and trips aggregating from forty-five to fifty miles, is about $4. This counts only those horses on actual duty on the road. The cost of motive power per car day for equal mileage in Richmond is less than $2 on the heaviest sort of grade work. The total operating expenses of a horse railway average for the five largest roads in Massachusetts 25.15 cents, and for all the roads in Massachusetts 24.7 cents per car mile; and the ratio of operating expenses to gross receipts is, for all the roads, 86 per cent.

The cost per day per horse, based upon the returns of four of the largest roads in New York, is 54 cents, and the cost per car mile from 9½ to 10½ cents per car mile.

In addition to the regular depreciation there is ever present danger of an epidemic in hot weather.

Since the cost of the motive power alone—that is, the cost of harness and stable equipments, horse-shoeing, renewals, provender, hostlers, etc.—is 40 per cent. of the total operating expenses, it will at once be seen how vitally important any material saving in the cost of motive power becomes. If, as we claim, the cost of motive power in an electrical system is one-half or less than that in a horse system, the percentage of gross receipts available for interest and dividends is more than doubled.

Furthermore, when we remember that the average running time for horses is only five to six miles per hour, we have another reason which constitutes an unwarrantable objection to the use of horses for rapid transit.

Among the systems which have been proposed to take the place of horses and which has given the most promise, and met with the greatest success where heavy grades and traffic exist, is the cable system. With its general features you are probably familiar.

There are, however, a great many objections. One of the most serious is the great initial cost of the construction and plant, the cost of the conduit alone ranging anywhere from $50,000 to $80,000 per mile of single track, making the investment so large that none but roads having heavy traffic can permit of the expense.

The conduit is necessarily large and costly in order to accommodate sheaves of sufficient size to prevent too great a loss in friction, and on account of the position of the cable and pulleys it is difficult to clean it. This large size also makes it difficult to construct a conduit which will not, because of the traffic and weather strains, be distorted and have the slots forced together or apart. The depth varies from two to three feet, more often the latter, and in any city street this is almost certain to interfere with steam, gas, or water pipes.

Each section of the line being a unit and operated from one cable, a break in the cable will suspend the entire traffic of that section.

A broken strand is apt to foul the grip and pile one car on top of another, as happened in Philadelphia, or suddenly start a car which is at rest, as recently happened on the 125th street line in this city. The cable is a heavy, cumbersome affair, 1¼ inches in diameter, and weighs about 13,000 pounds to the mile.

It is found that it is best to make these cables in 20,000 ft. lengths.

If a cable is stranded it cannot be used until repaired; if it is stretched 1 per cent. it is considered unsafe. These weaknesses of the cable system render it necessary to have a duplicate cable ready for instant use, and a double line of sheaves.

With a full load not over about 20 to 25 per cent. of the power of the engines is actually used for the propulsion of the cars, the balance being for moving the cable. With any light load the efficiency is even less, But it has been found advisable to have even greater power than this, most of the companies having from two to four times as much engine power as is required at any time, and from one and a half to twice the boiler power. This is necessary, because if any part of the cable engine breaks the whole section of the system is disabled.

Every turn in a cable brings a strain upon the grips and reduces the life of a cable, and any increase in the speed at which a cable is driven likewise shortens its life.

Difficulty exists in turning corners; expensive drums, and often times auxiliary cables being required. Constant care and attendance is necessary in lubricating the carrying and curve pulleys, and in taking care of the slots and switches—the estimates for Boston being about $3.20 per day per mile of single track, requiring the time of one man per mile. It is impossible to work turnouts, crossovers or sidings without auxiliary cables, or to operate single tracks.

A line once being put in, any extension is expensive and difficult because of the terminal machinery which is necessary, and the definite length of the cable.

The speed is limited; a car cannot travel faster than the cable and thus recover lost time, except on down grades; nor can it travel more slowly save by permitting the cable to slip. The regular speed then, must be the same in the most crowded sections of the street as in the less populated sections.

The starting is often with a jerk, and the whole motion of a lurching, unequal, unpleasant character, exceedingly disagreeable when first experienced.

Success in the cable railway system considered by itself depends largely upon the condition that a line of reasonable length shall be so located as to warrant frequent car service.

The chief advantage of a cable is, the cheapness as compared with horse power, of operating a heavily patronized road at a comparatively fixed rate of speed with no regard whatever to grade or gravity; but this cheapness is not inherent to the cable system, but due rather to the fact that steam has such a tremendous advantage over horses.

The motive power operating expenses vary from 50 to 70 of the cost of operating by horses.

Now, what is it electricity, by some of its systems, promises us for the ordinary street traffic? What are its advantages? First, it will do the work, and in a far more satisfactory manner and at a greatly lessened cost of operation than with horses. Electricity is simply an agent for which we have to pay a certain proportion to carry the power of our steam engines to take the place of the horse. Since not only on levels but also on the up and down grades an electric car can be run much faster than a horse car, and because it can be gotten under way and stopped much more quickly, the mileage with any given number of cars can be increased, the mean running speed increased, and the same time intervals made with a less number of cars; or, on the other hand, shorter intervals be made with a given number of cars. The equipment will occupy about 35 per cent. less space, the horse space being saved, and this fact, together with the ability to back when necessary, and to quickly gain headway, enables an electric car in narrow and crowded streets to work a passage through where horse cars would be at a dead stand still. Not only can cars run down grade faster, but also with much more safety, because of the possibility of the instant reversal of the motors in the event of the parting of the brake chain; and when arriving at the top of a grade, the faster a motor runs the better. It has not got to be held in to save it.

The riding of an electrical car is far easier than that of any cable or horse car, starting and stopping more easily, and being in a large measure free from lurching and oscillation. The cars are much cleaner. They can be brilliantly lighted, and they can be heated by electricity. There is no dust such as rises from the heels of horses. The sanitary conditions are entirely altered, and the health and comfort of the whole population is conserved. Stables with all their unsavory characteristics and the consequent depreciation of the value of adjacent real estate disappear. No sand is needed on the pavements, and the pavements are not so quickly worn out; often no centre paving is necessary. It becomes feasible to operate branch lines, and also combinations of grades, curves, and ill-conditioned streets which would be prohibitory to any other system. It is not to be claimed that any one system of electric traction has all these advantages; but in some form or other they will be found.

It has been said that an extended system cannot be operated by electricity, that the lines may break down, that a large number of cars cannot be operated simultaneously, especially when bunched up, that wires burn out, that brushes burn up.

True, these things have happened, and in case of defective workmanship or carelessness they may happen again. Although electricity is a force of unknown nature, and we know it by its effects, it is folly to hold that whatever faults in machinery may exist or accidents occur they do not come within a very narrow and limited category; and powerful and mysterious as it is, and answering an impulse with the rapidity of lightning, it is at the same time the most tractable and law abiding agent with which we have to deal.

There is not a freak of which it is capable which we can not guard against. It has not an attribute of which we cannot make positive use. It has no power of damage which cannot be controlled.

The development of its application, the mistakes which have been made and the accidents which have occurred are but the milestones and guideposts of the highway to success.

Were it not for the occasional failures there would be no advance.

As a matter of fact there is no apparatus for the transmission of energy that compares in simplicity and efficiency with the dynamo-electric machine and the electric motor. There is no machine in existence which, when thoroughly and well built, and decently cared for, is less liable to get out of order, and will stand harder usage and greater variations of work with a minimum of attendance and care. A motor has but one moving part, and that has a rotary motion. Its torsional effort under given conditions is a constant, and when properly built and proportioned some types have over a wide range the remarkable and unique quality of having this torsional effort vary as the square of the current used, a feature of marked importance in traction work.

As modern as the motor is, the peculiarities of all its phases are just as well known and capable of just as graphic representation and forecast, as are those of the steam engine. This can best be shown by reference to some curves which illustrate the characteristics of some of the Richmond motors, and have the same significance that an indicator diagram has for an engine. They are partly theoretical, and in practice are somewhat changed, but are interesting as showing a graphic method of motor indication.

For convenience these curves are plotted on both sides of the axis of X. The ampère turns on one leg of the field magnet are plotted as abscissæ. The curve $O\,A$ gives the mean characteristic of the machine when run as a series dynamo on a variable external resistance at 856 revolutions. $B\,C$ represents the equation of counter-electromotive force with an initial electromotive force of 400 volts. I have allowed an increase in the actual resistance of the machine of 10 per cent. for heating and other causes. The speed of the armature, the miles per hour made by a car with the Richmond gearing, the tractive effort in pounds for one motor, and the horse-power units are, for convenience, all laid off as negative ordinates. Equal units of horse-power and torque are laid off as abscissæ, so as to make the maximum points coincident.

The first important curve which we have to get is the speed of the motor in terms of its current. We have the law that the speed is directly proportional to the counter-electromotive force developed, and inversely proportional to the strength of the field. In other words, the speed curve $E\,D\,C$, in terms of the current, is plotted from the curves *O A* and *B C*, and the speed for any current is found by multiplying 856 revolutions by the ratio of the ordinates corresponding to that current.

The next curve with which we have to deal is that of the work of the motor in terms of the current; that is, the curve *O F C*, which intersects the axis of *X* at the same point as the curves of speed in terms of the current and the counter-electromotive force line.

A third and most important curve is that of the work of the motor in terms of its speed, which is the curve *O D G* plotted from the curves *E D C* and *O F C*, with the horse-power units transferred to the axis of *X* as abscissæ. For instance, following the negative ordinate for 30 ampères, we find, at the proper intersection, a speed of 360 revolutions of the armature, or 4.82 miles per hour of the car; and, at the insection of the work curve, that 11.65 h. p. is developed. These two values give us the point *S* as one of the points of the work curve, and so on.

Referring to the work required to be expended on a car, we have from the formula—

$$\text{h. p.} = \frac{w\,n}{335}\left(\frac{k}{2000} \pm \sin\theta\right)$$

where w = wt. in lbs.

n = miles per hour,

k = resistance to traction in lbs. per ton on level,

θ = angle of grade,

the work which is required for any grade, speed and weight. Hence for any given conditions we can plot the equation h. p. = $a\,n$, and by its intersection with the curve *O D G*, find at what speed the car will be moved by the motors and what current is required if we know the gearing. On the other hand, if we assume that a certain load on a certain grade shall be handled, and we elect that it shall be done with a certain efficiency of the motor, or on a certain part of the work curve, we can draw the car curve for this intersection and then deduce the speed of the car and, from it, what must be the ratio of gearing to enable the car to be handled by the motors under these conditions. Four of such car curves are drawn—*O R*, which shows the maximum which could be moved; *O M*, which shows what can be done at 50 per cent. efficiency; *O N* and *O P*, showing the equations which are constantly in practice.

In the actual computation 10 per cent. extra is allowed for losses from friction and so forth.

With the Richmond gearing, and a car equation of h. p. $= 4.8\,n$ we would have a car weighing 14,700 pounds sent up a 10 per cent. grade at 4.82 miles per hour, and each motor would take 30 amperes of current. If the grade or weight were increased, the curve *O N* would be carried toward *O M*; or, if the ratio of gearing is changed, then with the curve *O N* in its fixed position, the weight of car or grade could be increased, although the speed, of the car would be reduced. Other curves are shown, *L K* showing the tractive effort of the motor in terms of its speed, and *O H* showing the tractive effort in terms of the current; the tractive effort varying, of course, as the work divided by the speed.

The variety of problems which can be worked out from these curve sheets is very large, and they become of vital importance in dealing with any new work. Nothing could show more clearly the value of a mass of iron in the field magnets (as I have always held to be the proper method of building motors), and the exceptionable qualifications of motors for tractive purposes. Any change in the switch or any change in the potential will vary these curves materially. One fact, however, is to be noted; no matter how the field may be changed, the speed of the motor at 50 per cent. efficiency is nearly constant.

The connection of the motor to the driving wheels has been attempted in a great variety of ways, and nearly every mechanical device possible has been used. Often the motors are carried upon the car body and connection made with the wheels, which should bear a variable position relative to the car, by means of friction wheels, sprocket-wheels and chains, or by belts, ropes or wire-chains with tightening devices. The motor armature has been mounted directly on the axle, and also hung on a frame between the axles, being connected to both wheels by cranks and side rods, necessitating equal speeds of revolution of the motor armature and the wheels.

These methods are all crude and unsatisfactory. They are makeshifts and ignore the most important and essential facts which have been developed in car and motor construction. There is to my mind only one good way to connect a motor, and this is to centre the motor upon the axle, supporting it flexibly from the car body or the car truck, and driving by gearing with one or two reductions according to the service which is demanded. This is the only way in which an absolutely safe and positive connection can be depended upon both for ordinary work and for the normal and sudden strains called for in an emergency.

Properly made, the friction of this method of reduction is very low, the motion is smooth and free, and reasonably noiseless.

The proposition to couple the motor so that it shall make the same number of revolutions as the axle ignores one of the most important points which makes light weight in motors possible, and that is the high speed at which the armature can be run.

I have pointed out, in describing the curve characteristics of a motor, one of the essentials to determine the proper ratio of reduction for any motor when required to do a given work. I may here add a brief formula, which will also serve as an index in this matter: Let f equal the maximum velocity in feet per minute at which an armature at any time is allowed to travel; n the number of miles per hour to be made by the car; D the diameter of the car wheel in inches; d the diameter of the armature in inches. Then the ratio of the gearing must be expressed by the equation.

$$R = \frac{f\,D}{88\,n\,d}$$

For instance, let $f = 4{,}000$, $n = 12$, $D = 30$ and $d = 10$; about the values which exist in street car work. Then

$$R = \frac{4{,}000 \times 30}{88 \times 12 \times 10} = \frac{125}{11}$$

and this should be made in two reductions. As in our largest trucks, let $f = 4{,}000$, $n = 35$, $D = 42$ and $d = 18$. Then

$$R = \frac{4{,}000 \times 42}{8 \times 35 \times 18} = \frac{100}{33}.$$

In the application of electricity we are limited to no one system; we have a choice which will permit us to meet any exigencies or conditions of traffic. There are two kinds of application.

In one, each car is perfectly independent of the other, receiving its charge at the central station and acting as an individual unit. The storage battery method is such a system.

The other is a system of direct supply, and, unlike the battery system, where the energy of the steam is four times converted, it suffers only two losses. In this system the current is delivered directly to the line, from which the motors as they move along derive their current. There are several forms of conductors, centre and side rails, the main tracks, conductors carried in conduits, and overhead. Either a complete metallic circuit may be used, or one conductor with the rail and earth return. The machines can be operated under three different systems—the series, multiple series and parallel circuit distribution.

Of these various plans, I shall discuss only three—the storage battery, the conduit and the single overhead wire system with underneath contact carried on the car, and for the systems of distribution the parallel circuit system only.

The storage battery from the time of its first introduction to the public has offered a tempting solution to the application of electricity for surface transit. The great advantage of the battery system is that while most of the advantages of an electric system are retained, such as perfect control of the car, ease of manipulation, cleanliness and so forth, each car in itself becomes an independent unit not dependent upon any general accident to a system. As attractive a solution as the storage battery at first sight seems, there are many points of disadvantage in the present state of its developement. One of the most important is the excessive weight of battery which is necessary under the conditions of service which are met with in this country, from 3,400 to 4,000 pounds being the ordinary weight. Not over 10 per cent. of the lead in these batteries is actually used for storage. On account of the weight and the room which the battery takes up, there has to be a radical change in the construction of the cars, and while old cars can be made use of by a great deal of patching, it is not advisable to attempt this. This change makes the present cars useless, and likewise increases the cost of new car bodies.

Then again, while the tracks as they exist may stand the additional weight of motors on a four-wheel car, the additional weight of battery, making the total weight on a given wheel-base double what it was before, will render it necessary on a great many tracks to relay the road-bed, or to put in 8-wheel cars. Of course these objections are not so serious on a new road as they are on an old one.

But I confidently expect to see in the not so very distant future, weight of battery necessary to handle a car reduced one-half; this, besides overcoming some of the objections which exist now, will halve the expense of handling them at the central station, and will also allow the present standard width of aisle to be maintained.

The present excessive cost and unknown depreciation of the battery, its lack of efficiency, limited capacity, the fact that only a portion of the charge can be taken out with safety, and the reduction of its output when sudden demands are made upon it, are all of such a character as to leave wide room for improvement. The loss of efficiency in conversion I do not esteem of such vast importance, because, in any large system where the engines are kept going under a steady load, the cost of operation will not be so very much more than where operating under continually varying loads. The limited rate of discharge, as well as the limited capacity, limits the grade work to which a battery car can be subjected. For instance, as batteries and cars are now made, we can easily have the following combinations of weight:

	Pounds.
Weight of car	5,000
Batteries	3,600
Motors and other apparatus	2,000
Sixty-five passengers	9,100
Total	19,700

To start and to accelerate this load on, say a 9 per cent. grade, demands a discharge rate which no storage battery company cares to recommend.

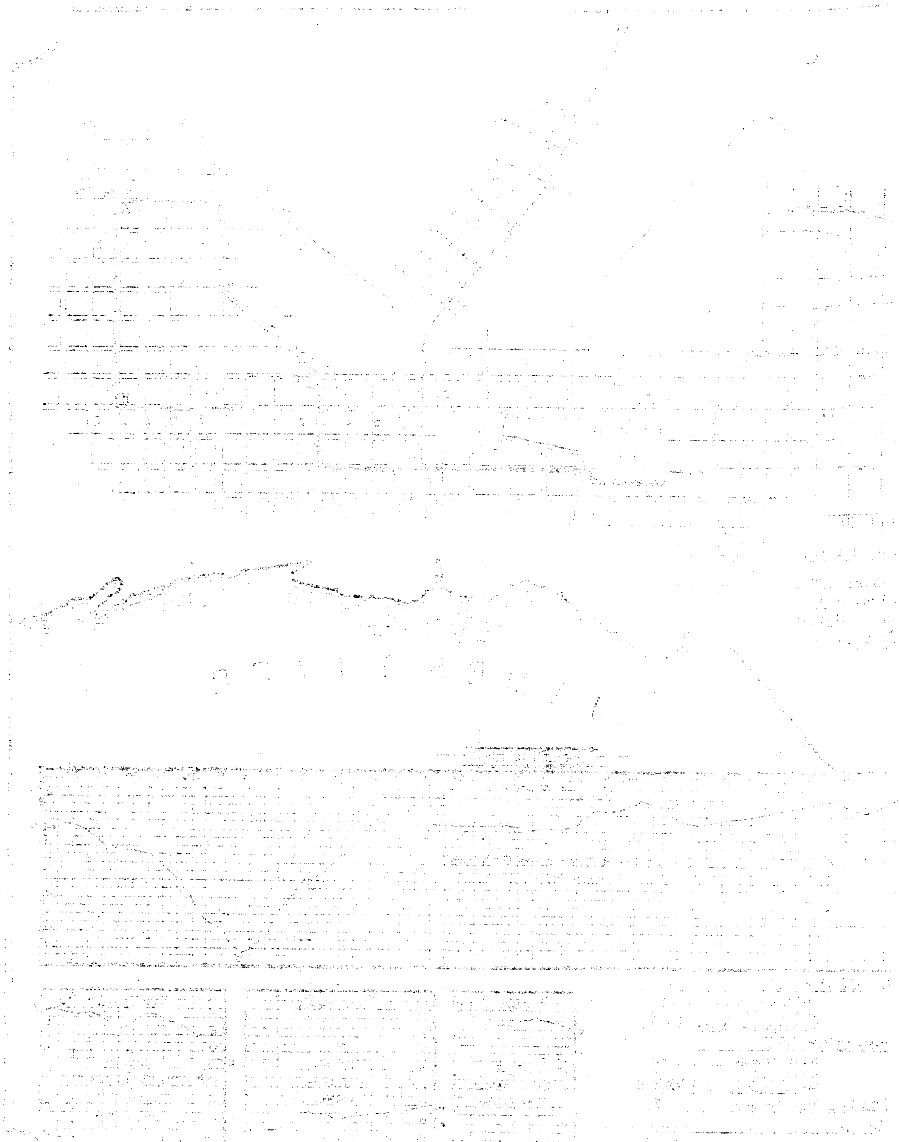
As near as it is possible for us to get at a correct estimate on a new road of moderate grades a storage battery system could be operated at about 80 per cent. of the cost of operation by horses. This is about double the cost of the direct system of supply on a large scale.

But the storage battery is not going to stand still.

Many men with abundant capital are striving for improvement, and I think there is but little question that batteries will be reduced in weight, increased in capacity, and made far more efficient and long-lived.

The conduit and overhead systems have one point in common. Both are dependent upon a central station, and the actual cost of operation (except for the care and maintenance of the conduit itself) would be about the same. There is, however, a great difference of investment. Both must have about the same amount of copper, and, to be effective, about the same distribution.

The difference of cost then is practically the difference between pole erection and conduit laying, and this will be from $25,000 to $30,000 per mile. As an investment of course it means that with the conduit very much more money has to be earned. The



electrical conduit can be built for less money than a cable conduit, because it can be made of less depth, but it cannot be built without due regard to the strains of street traffic and weather, and with proper drainage and cleaning facilities. The advantages of the conduit are chiefly that there are no overhead obstructions. Its disadvantages are, besides its great cost, the lack of cleanliness, increased possibilities of leakage, and greater difficulty of repair in case of accident. Recognizing, however, the demand which exists in certain places for a conduit, we have developed such a system which has many of the same elements of simplicity and reliability that our overhead system possesses.

The working conductor carried in the conduit is divided into sections, which at intervals of every block or half block, as desired, are connected through automatic cutouts in manholes to the main conductor, which is continuous and enclosed in a lead or iron covered cable. Any accident to a section, such as flooding, short circuiting, and so forth, will simply cut out that particular block.

For the immediate solution of the question of more rapid and cheaper transit in many places the overhead system is most available.

I am aware that a great hue and cry has been raised, especially of late, against any electric wires whatsoever being used above ground. The feeling which exists is not entirely unreasonable; in fact it is easily accounted for by the most casual inspection of the condition of the wires of this city. But many of the objections which are raised against all overhead systems are not necessarily inherent; they are very largely due to gross carelessness in construction engendered by a faulty municipal administration. We have in our cities little or no active control or over sight of the wires which have been put up in our streets. Year after year, from the earliest telegraph days, those in control have granted to telegraph, electric light, telephone and electric power companies innumerable more or less limited franchises for stringing or hanging wires through our streets. These permits have often been granted without any restriction whatever; they are often granted for a "consideration." The erection and maintenance of these lines being a large factor in the cost of construction and operation of the companies, they have been put up with little or no regard to public convenience or safety—certainly with no regard to any æsthetic tastes—and often times with an utter ignorance of the proper methods of construction. Fire-underwriters in their wisdom, have demanded an insulation to stand fire, forgetting, or in apparent ignorance of, the fact that protection from moisture and leakage is of far more vital impoetance. Hence we have that treacherous wire known as "underwriters' wire," or more commonly among electricians, "undertakers' wire."

The poles have until lately, been of the cheapest construction, badly set and continually marred and weakened by the spurs of the linemen. New lines have been continually added, and poles intended originally for a certain number of wires are now carrying double or treble that number.

The electric light companies, after cutting out a lamp for which a contract has expired, leave the bights of their wires hanging in loops. The telegraph and telephone companies allow thousands of miles of "dead" wires to remain overhead, or to hang in festoons from every pole, which in some sections are utilized as clothes lines or are used by the street urchins as kite traps.

They cross and recross the streets at every angle, every elevation, and at every tension. They are strung surreptitiously on poles to which no right exists, and over buildings for which no permit has been granted.

No one seems to have the power to compel a change or improvement in these practices, and the companies themselves never will do it until so compelled. The sharp competition in the lighting business, the small profits derived therefrom, the great trouble of hauling down and getting rid of miles of dead telephone and telegraph wires, all these interfere with a satisfactory straightening out of the existing system of overhead wires.

In all movements for public improvement we have to consider the arguments for, as well as against such improvements. It is not sufficient that we say that there are disadvantages attending the introduction of the system, we must consider likewise the possible advantages; and this is especially important when we consider the use of the overhead line in a system for propelling street cars. Such a line being part of an investment of a company which has practically no competition on its own streets, and whose own interest would require it to keep its line in first-class condition, can be put up in a manner presenting few of the objections which are generally urged against a system of overhead wires. I would not urge its adoption for all places, but in all suburban districts, for comparatively narrow streets, for all streets operating under an elevated railway structure, or where the tracks are near a sidewalk, the direct system of supply from an overhead line with underneath contact, presents advantages which, once appreciated, far outweighs the possible objection of such a system.

Several methods exist in which the wires can be erected. One in which span-wires, at a distance of 125 feet apart, reach across the street from building to building, or from pole to pole, and have suspended from them directly over the track, at a height of about 19 feet, a longitudinal wire of not more than a quarter of an inch diameter, this wire being termed the "working conductor." This should be of the very strongest character. It should have a high elastic limit, great tenacity, and resist corrosion and oxidation.

Its conductivity is a matter of less importance, since not upon it, but upon the main conductor which supplies it do we depend for the main transmission of our current. Hence the elements of strength and reliability are of far more importance then conductivity. We have found that the best wire for this purpose is that alloy known as "silicon bronze"—a wire which can be drawn for a tensile strength of from 75,000 to 150,000 pounds per square inch; that is a strength higher than that of the best commercial steel. Such a wire it is almost impossible under conditions of service to break, and its life is very long. Samples of hard drawn copper wire, which possesses only from one-third to one-fifth the tensile strength of this wire, show, after months of severest usage, scarcely any signs whatsoever of abrasion, or reduction in size. This wire is of the same size whatever the distance run or the number of cars operated, and the main conductor may be stretched along the buildings, through a side-alley, along poles, underneath the elevated structure, or buried underground. In the latter case there would be in a single track street only one longitudinal wire, and that one over the track, and it would be no longer than a single telegraph wire; and in the case of a double track, there would be only two wires stretched longitudinally along the street, and these would be as taut as harp strings.

Switches and cut outs are put in main and working conductors in such a way that the different sections can be operated independently, and cut out automatically or at will.

The span-wires can be as much above 19 feet from the ground as is desired. Of course, where a telephone or electric light line crosses the street and may break and fall upon such a conductor, it would be necessary to have one or two guard wires above it.

These guard wires can be very properly carried from the span wires themselves. Furthermore, if desired, the stretches between the span wires where guard wires are used can be very much longer than 125 feet; they can be even as long as 500 or 600 feet—that is, two or three city blocks—in which case the two guard wires form a light suspending structure from which, at intervals of a hundred feet, the middle wire is carried in a straight line.

In very many cases it is unnecessary to have any span wires whatsoever. In those streets where the track runs near the curb a light pole with an ornamental bracket is all that is necessary.

In a broad street where the tracks occupy only a small portion of the street, and are near together, a line of poles of ornate design with light arms projecting on either side can follow the centre of the street. Oftentimes there is a middle division or park, such as exists on the Boulevard in this city, which will afford space for the poles without interfering with anything.

These same poles can be used for lighting the street.

On streets occupied with elevated railroad structures, the structure itself can be used as a support for the overhead wires.

Among the numerous places in New York where an overhead system could be put in perfect operation are Eighth Avenue, from Fifty-ninth Street up; the Boulevard from Fifty-ninth Street up; a part of the First and Second Avenue lines; the Third, Sixth and Ninth Avenue lines, and all the suburban extensions in the annexed district.

To avoid any extra strain on this wire, the current is taken off by a flexibly supported trolley carried on the car, and making contact underneath by upward pressure, as will be more specifically described later.

With such a system put up in the proper manner, it is almost impossible for any accident to occur which will disable the line or prove a hindrance or annoyance to the public in any way.

The wires are perfectly clear of the operations of the firemen, and should they in any place interfere, the obstructing section can easily be cut out without disabling the remainder of the system.

The question of the "electromotive force," or electrical pressure, so to speak, to be used, is one of great importance. I do not favor going above about 400 volts, and this on a continuous current circuit. This is less than one-seventh the electromotive force which exists on many of the arc-light circuits in this city; and the current being of a perfectly smooth and continuous character is free from the danger which might be feared from a current of equal electromotive force of the alternating or pulsating character.

In point of fact, in Richmond, a very large number of our men have received the full E. M. F. from our line without any injury.

On a dry day, the trolley wire has lain coiled up on the ground while the cars are in operation; and while certain changes were being made in the overhead circuit, the men worked continually with the current on the line. I have never known a constant potential current of 400 volts to cause of itself the death of or serious injury to a human being.

The past year to me has been one of great responsibility and wide experience. It has been a hard year. But the experience and expense are as nothing compared with the great value of having been with my associates instrumental in bringing to a successful issue one of the most important pieces of electrical engineering with which I have ever had anything to do. Among the roads which we have equipped the most important is, of course, the Richmond Union Passenger Railway, which now has six times as many machines in operation as any other road of its character in existence, at which every difficulty to be encountered in an overhead system on heavy grades and with sharp curves has been met, and which has for all time conclusively settled the question of the economical operation of a large road by the method of direct supply;

I will briefly refer to the road, giving some illustrations of its principal features, and some facts about its operation.

Some idea of the character of the road may be gathered from the following: The total trackage is about twelve miles. It runs a very irregular course and reaches the principal parts of the city. About nine miles of street are covered. The central section is a double track for a distance of something over two miles a part being laid on paved streets, and the balance on macadam or unpaved streets. All the extension and branch lines are on unpaved streets, many of them in clay soil, which in wet weather covers the tracks, and where it would be an impossibility to operate horses without paving. The double track section partly encircles the old State Capitol, and presents difficulties rarely equaled elsewhere. This part of the line in a distance of less than one thousand feet has on both east and west-bound tracks, four curves, the inner rails of the west-bound track being of about 27, 30, 40 and 50 foot radius. The power necessary to run these sharp curves is much increased by the fact that grades are encountered on the curves as high as eight per cent., and the lay of the streets has required some of the outer rails to be some inches below the inner one. Running eastward from the Capitol the road descends to a low valley through a series of grades varying from three to ten per cent, and then ascends a series of sharp grades and curves in a new district to one of the car houses.

Going west from the State House the road rises to nearly the highest point in the city, and then runs through a residential district across the Richmond, Fredericksburg & Potomac Railroad, and after several sharp curves extends back a further distance of two miles to a second car house beyond the new reservoir. Two branches connect with its main line, one leaving at the old Market Place crossing, and paralleling the Chesapeake & Ohio Railroad, and terminating at its machine shops. The other runs into what is popularly known as "Africa," a section almost exclusively inhabited by colored population.

The narrow streets, their hilly character, and the lack of paving, especially in the new districts, has made the road one of exceptional difficulty to operate. There are no less than twenty-nine curves requiring bent rails, the majority being of sharp radius, five being less than thirty feet. There are also several sweeps or curves of larger radius, several turnouts and cross-overs. The grades are the maximum ever ascended in practice by a self-propelled car depending on track adhesion alone, and many of these grades are very long. Going east there is one section of almost continual ascent of about 5,600 feet, with grades varying from three to seven per cent., and going west there is a stretch of 1,900 feet, with grades varying from three to ten per cent.

When we first undertook to equip this road in the face of these difficulties, there was the additional discouragement of the ridicule of a great many prominent street railway men and car constructors as well as electricians. It was only a comparatively short time ago that one of the leading lights in street railway cir-

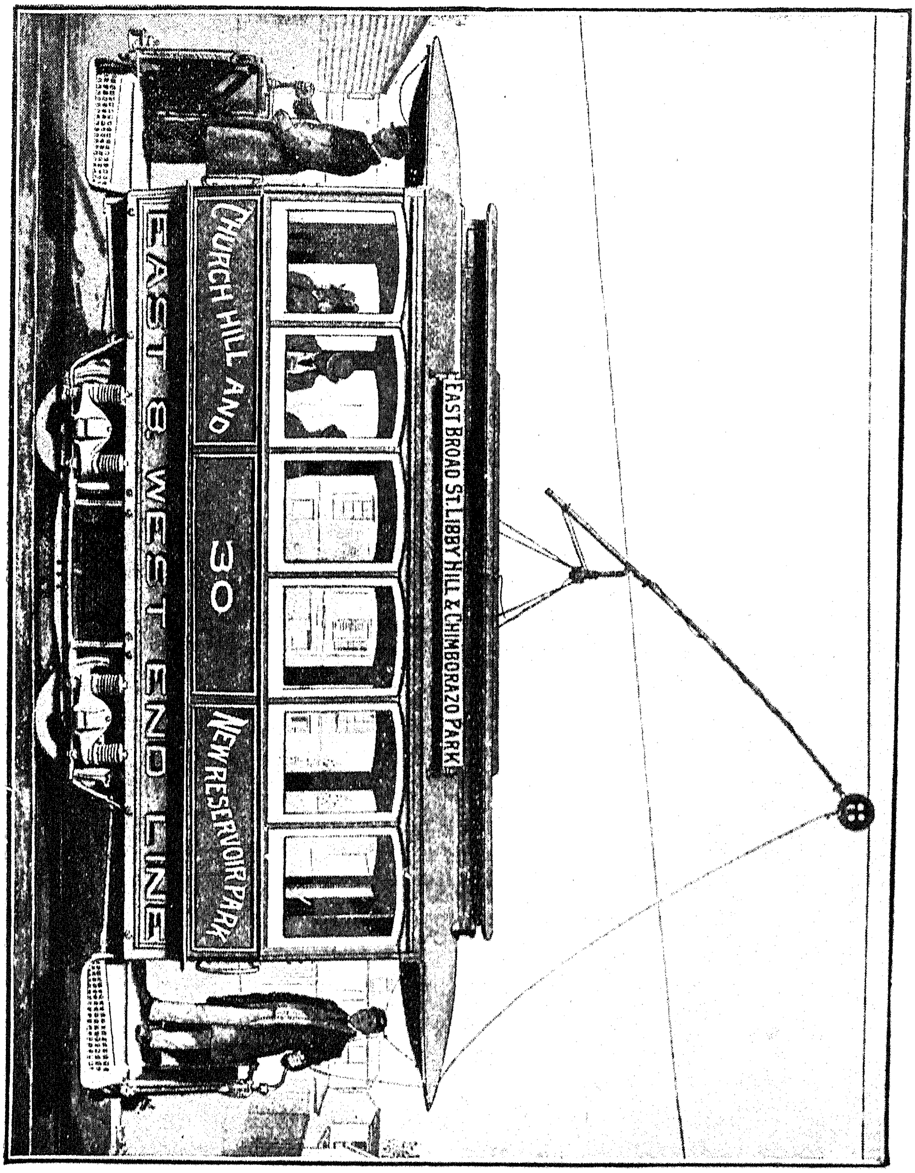


Fig. 3.—CLOSED CAR ON RICHMOND, VA. RAILWAY

cles made a report to the street railway convention in which he stated that unless some new invention should be made it would be impossible for any self-propelled vehicle to satisfactory operate under the ordinary condition of street-car service. He, as others, held that the grades and curves met in practice were so severe and the condition of the track so bad that a car propelled by its own wheels could not under any possibility, day in and day out, be relied upon to move itself. One electrician even made the statement only a short time ago that any man who should attempt to operate on more than a six per cent. grade would lose his reputation.

Reliability, economy, simplicity of construction and operation on a scale never before attempted and under conditions which we could not foresee. This was the problem, and perhaps it was well we did not know the difficulties. Then again, while there have been strong friends of the enterprise in Richmond itself, there was a class that felt anything but pleasure at a stranger's advent.

The change of feeling with which this enterprise has been regarded by this doubting element is somewhat significant. When the franchise was originally secured the city was blest by one car-line only, and this had dingy bob-tail cars drawn by mules, which, on the heavy grades characterizing the topography of Richmond, made very slow time. The new districts were not reached at all. When the present line was proposed there were not wanting philosophers who knew that no road would be operated. Even when the tracks were down these same wiseacres predicted that no cars would ever move over them, and most certainly if they did, the time-honored mule would be called upon for duty. Even after the cars were put on the line experimentally there existed but little faith that success would be reached. But times have changed. The mules on the competing line are to give way to electricity. The citizens of Richmond are the strongest friends of the electric railroad to-day, and as regards the street railway community elsewhere, the work which the road has done and is doing and the acceptance of the contract, speak for themselves.

The difficulties we have met are many. One of the most annoying at first experienced was that the cars frequently jumped the tracks. A large portion of the track was, as has been stated, laid in macadam and dirt streets, the latter being chiefly in new districts and in a soil of which the chief component was red or yellow clay, and although the curves were of sharp radius, only one guard rail was used, and this was not bound in by paving. The winter months brought frequent and heavy rains. The tracks were found to be settling out of sight in mud and especially at the curves they were spreading. The cars being large and heavy, with a 4 foot 8½ inch gauge and a 6 foot wheel base, helped to make the curves spread by the tangential pressure of the forward wheels on the outer rails. Mud and stones would get into the grooves, and the cars having a tendency to go ahead instead of being pulled around by the head as with horses, would climb the track, and before an inexperienced man could check them, the wheels were down five inches in the mud alongside the stringer pieces and the track brakes, hard and fast against the tracks. Sometimes it was lifting, sometimes prying, and sometimes a heavy hard pull on the part of the motors to get them back again. Experience, however, is a great teacher, and among other things which has been taught is this: that money will be saved by good track construction and the observance of a few practical points. All curves should be bound in and paved, and this has since been done in Richmond. They should likewise have two guard rails, because it is much easier to steer a car by its rear wheels by holding them in place, than by allowing them to crowd the inner rail and force the forward wheels to climb the outer one. In dry weather, with the large wheel base and sharp curves, it is found advisable to grease the grooved rails.

Another point which has to be carefully taken care of is the character of switch which is put in for sidings and cross overs. If drop switches are used, they should be of such character as to allow the car to come down easily; tongue switches are now being put in on all the new work which is being done. It is a rare thing at present to have a car leave the track. I have seen a car in the winter propelled for a hundred feet off the track, and also leave and go back on its own account.

The power house is situated two blocks away from the nearest point of the line, and is almost equi-distant from its extremities. As a distributing point, it is, therefore, both theoretically and practically, a central station. In the construction, no expense has been spared, and the performance of the station has justified it. The battery consists of three cylindrical return tubular boilers of the latest pattern, mounted on a Jarvis patent furnace, their capacity being 125 h. p. each. The water is taken from the city

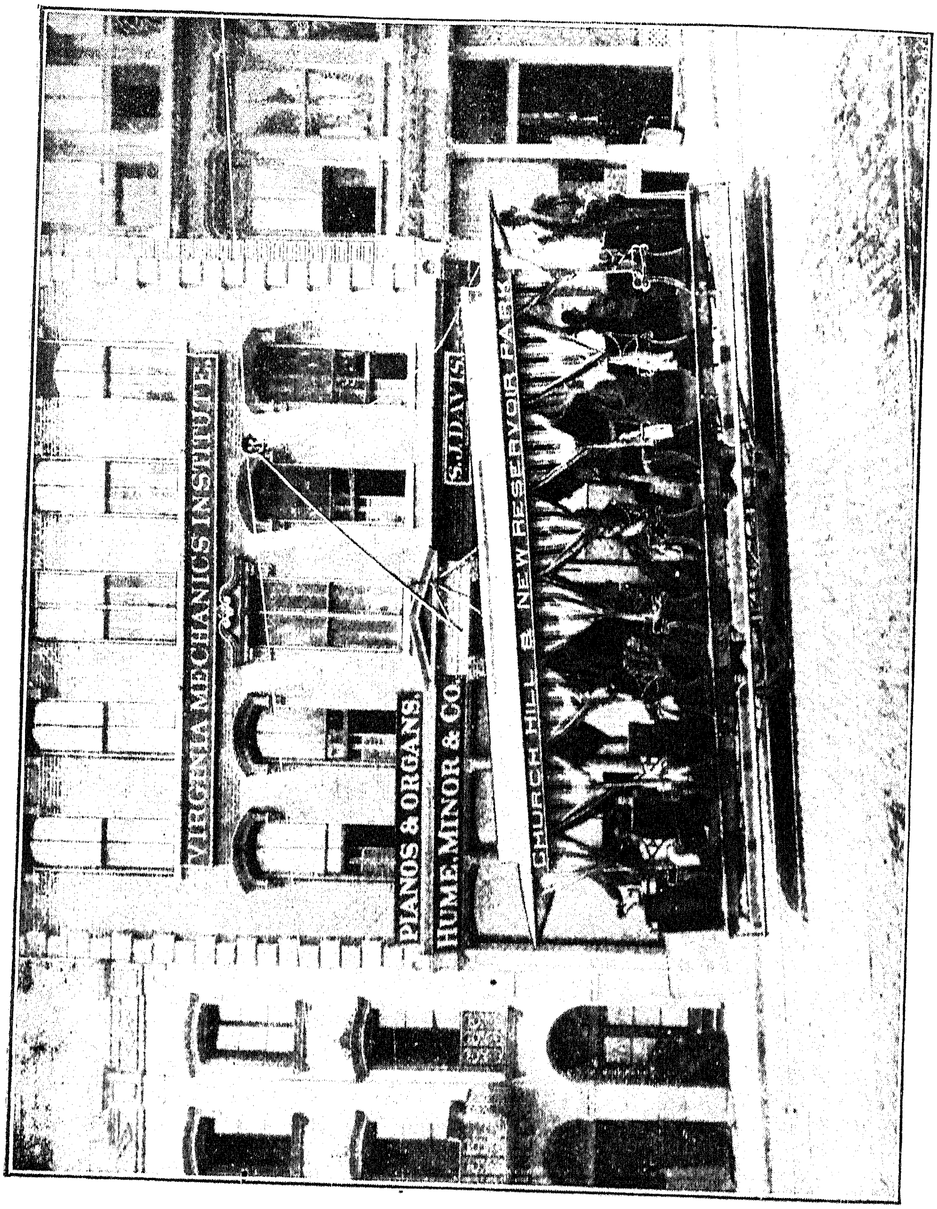

VIRGINIA MECHANICS INSTITUTE.
PIANOS & ORGANS.
HUME, MINOR & CO.
S.J. DAVIS
CHURCH HILL & NEW RESERVOIR PARK

FIG. 4.—CAR ON STEEP GRADE, RICHMOND, VA.

mains or from a well. All feed-water connections are of brass pipe. From the pumps the water passes first through the feed-water heater, where it is heated from 150 to 200 degrees F. From there it goes to the filter. The draft in the furnaces is regulated by an automatic steam damper regulator, which with anything like regular firing, keeps a pressure within two pounds of that for which the regulator is set.

The boilers all feed into one main feed steam pipe, which in turn leads to a separator, a large iron cylinder into which the steam passes, the water falling to the bottom and the dry steam being carried off at the top. All live steam pipes in the boiler room are covered with magnesia.

The present battery occupies less than half the available space in the boiler-room so that four more boilers can easily be put up when it becomes necessary to enlarge the plant. Two more have just been added. An important feature of these boilers, which are the source of power of the whole system, should not be overlooked, that is the quality of coal burned. Where a very perfect combustion is obtained a grade of coal can be used which would be impracticable in an ordinary furnace; the mixture at present being used is two parts screenings to one part bituminous, costing from $1.75 to $2. per ton delivered. As the station is only three hundred feet distant from the depots of three railroads, coal can be obtained almost at the door. From the boiler-room both the steam and exhaust pipes run in an underground, mineral-wool-packed trench to the engine-room.

The railroad engines of which there are at present three, are of the Armington and Sims high-speed type, and develop when running at full load 125 horse-power each at 250 revolutions.

Each engine drives two special Edison dynamos of 40,000 watts capacity each, wound for 500 volts maximum pressure. The dynamos feed into copper "bus" bars, supported on the walls by porcelain insulators.

Each machine has its independent ampère meter, and in addition there is a general ampère meter at the end of the positive "bus" bar. From this bar the current passes to four snap switches, each switch being connected through a three-plug safety switch block to one of the feeders supplying current to the main line wire. These four-feeder wires tap into the line wire at different points, thus maintaining the pressure approximately equal all along the line. At the ends of one of the feeders in the central station, pressure indicators are to be attached which will indicate the voltage at the junction of one of these feeders with the main current wire, and this will serve as a guide.

The electric circuit consists of two parts—the overhead and the ground circuit, each being of compound character. Along the curb stones at distances of 125 feet are thirty-foot poles, inserted into the ground a distance of five feet. Great difficulty was experienced at first on account of the clayey character of the soil, and it became necessary to put in very heavy poles at the corners, which were imbedded in concrete and stones to prevent settling out of place after heavy rains. These poles carry the main circuit which extends throughout the entire length of the roads, and is a copper wire three-sixteenths of an inch in diameter; this is called the main conductor, for on it depends the operation of the road in case of accidents or repairs to the contact wire.

The arrangement of main and working conductor are as already set forth.

This main conductor is, as stated, supplied at widely separated points by feeders, which come from the main supply at the central station. Without the use of these feeders, which can also be reinforced if necessary, the size and cost of the overhead conductors would be very largely increased. The curves are formed of a series of short chords, which approximate the central line of curvature.

The current is taken from this working conductor by a light structure on top of the car. This consists of a low skeleton framework which carries an adjustable swivelling trunnion, having at its upper end a jaw in which is suspended a counter balanced trolley pole, having at its extension a grooved wheel held by a spring against, and making running flexible contact with, the under side of the working conductor. The flexibility of this arrangement is very great, it being able to follow with perfect facility variations of the trolley wire two or three feet in either horizontal or vertical direction, at any speed, and around any curve. What is all important, it relieves the line of strain instead of adding to it. When getting to the end of the track this trolley trunnion is swung around so as to trend in a proper direction abaft the centre of the car. If off the line it can be replaced from the car quickly and easily in the darkest night. From the trolley the current passes through a protected wire to the lightning arrester and thence to two switches, one at each end of the

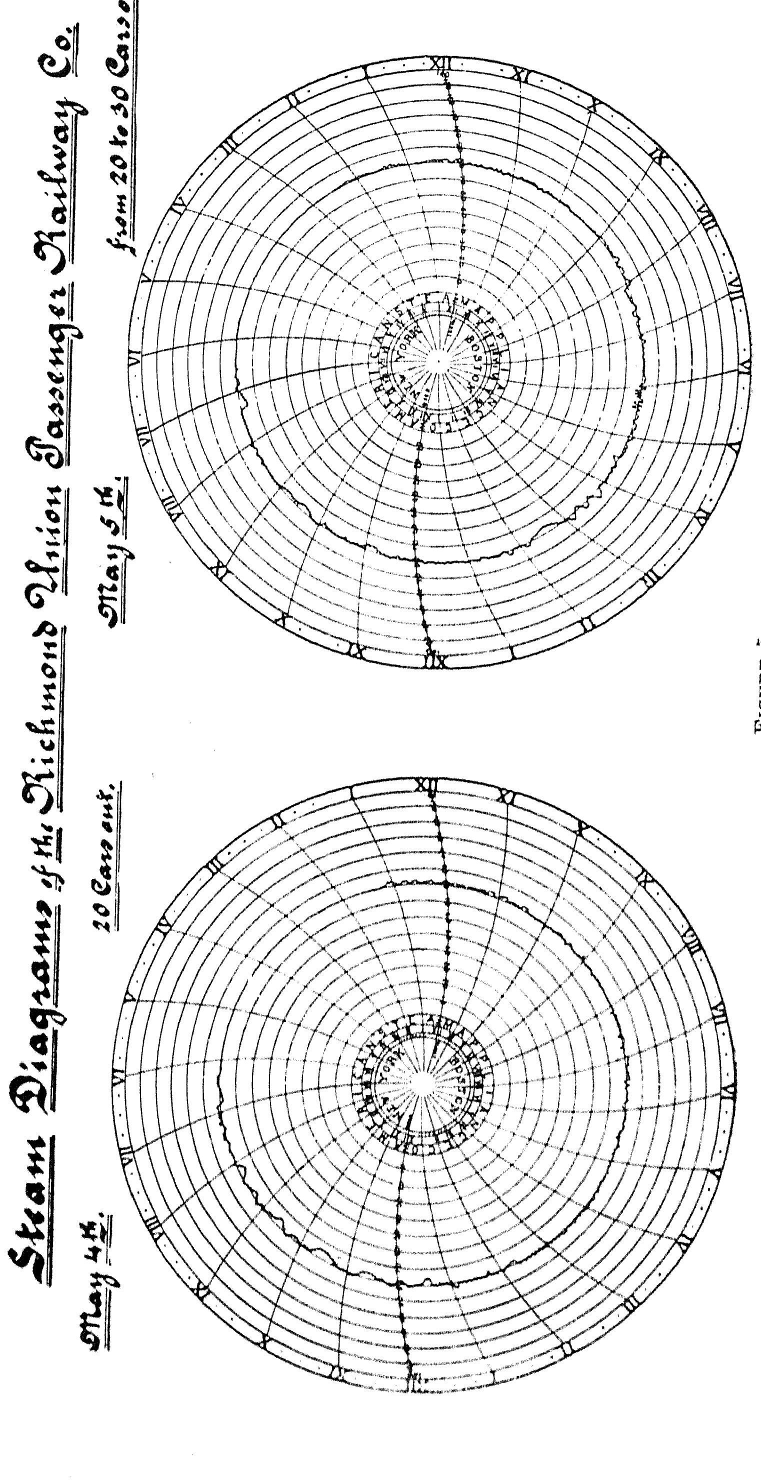

Steam Diagrams of the Richmond Union Passenger Railway Co.
May 4th.
20 Cars out.
May 5th.
from 20 to 30 Cars out.

FIGURE 5.

car, whence after whatever commutation is necessary, it goes to the motor circuits, and thence to the wheel frame.

The return circuit is through the track, and thence by both metallic and ground circuits to the station. Each section of rail is joined to a copper ground wire, which rûns throughout the length of the road underneath or alongside the stringer pieces, which is to prevent appreciable differences of potential existing between the rails; as a further precaution, at intervals of 500 ft. this ground wire is connected to an earth plate, and at seven points widely distributed, these ground plates are supplemented by heavy iron pipes sunk into iron ore, or 12-inch water wells, about 25 ft. deep. The ground wire is connected to the station, and there is also a main ground connection made there in a thirty-foot well through a large sink plate, and another to the entire water-works system. Some modifications of the overhead system are now being made which will make it impossible for any accident to one section of the line to disable the remainder; will enable the different sections to be operated independently; and, in case of a fire which may necessitate the blocking or temporary disabling of one part of the line, will allow this section of the line to be cut out of action either automaticaly or at will.

Each car is fitted with a duplicate and very powerful motor equipment. This is carried entirely beneath the car body, is very compact, out of sight, and necessitates no radical change in the construction of the car. The body had to be stiffened a little and 3-inch steel axles put in. One of the great objections which have been met with in applying motors, whether steam, electrical or of any other character, to the propulsion of street railways, has been the difficulty of getting sufficient track adhesion to operate the car under all conditions of track, loads and grades. Especially was this objection true in Richmond, where it was manifest that no application of a moter system could be fully successful unless the entire weight of the car and its contents was made available for traction.

One of the manifest reasons is, of course, the slippery character of the track under many conditions. Another thing should also be noted, and that is that it is almost impossible in practice to cast all wheels of exactly the same circumference, or at least so that the paths described by the lines of contact shall be the same. Moreover, on curves, the wheels are all traveling over paths of different lengths. Theoretically, the wheels should be independently driven, but since this is not possible it follows that the next best thing is to drive each pair of wheels independently. It is a fact which admits of no dispute that there are conditions of track which will not permit of a car being driven from one pair of wheels only, and this difficulty is often augmented by the fact that if one pair of wheels only is used they will sometimes be leading, sometimes following; on heavy grades with one platform reserved for the driver there is a very serious displacement of the centre of gravity and there may be very much more weight upon one pair of wheels than on the other. The whole experience in Richmond has been to emphasize the value and necessity of independently driven axles.

To accomplish this, one motor is centered upon each axle, and to allow the required freedom of motion, and at the same time to preserve perfect parallelism in the meshing of the gears, and also for taking part of the weight of the motor off the body of the axle and to throw it on to the journals, one end of the motor is supported by double compression springs playing upon a loosely suspended bolt, which is supported from a cross girder in the bottom of the car.

The motors are, then, so to speak weighted or flexibly supported from the car body, and the motion of the armatures is transmitted to the axles through intermediate gearing, of compact form and great strength, so that whenever the axles are in motion there is a spring touch of the pinions upon the gears. Barring friction a single pound pressure exerted in either direction would lift or depress the motor a slight amount, and no matter how sudden the strain, whether because of a variation of load or speed, or a reversal of direction of rotation, it is impossible to strip the gears unless the steady strain exceeds the strength of the iron, the pressure on the gears always being a progressive one. The motor armature makes nearly twelve revolutions to each revolution of the car axle.

The motors, although in one sense independent, are connected with duplicate switches at either end of the car which afford simultaneous control of both machines. These motors, which are of normal capacity of 7½ horse-power each, can for a short time work up to 30 horse-power, and are capable of producing a tractive effort of over three thousand pounds at 50 per cent. efficiency.

The machines are controlled by a single movement switch

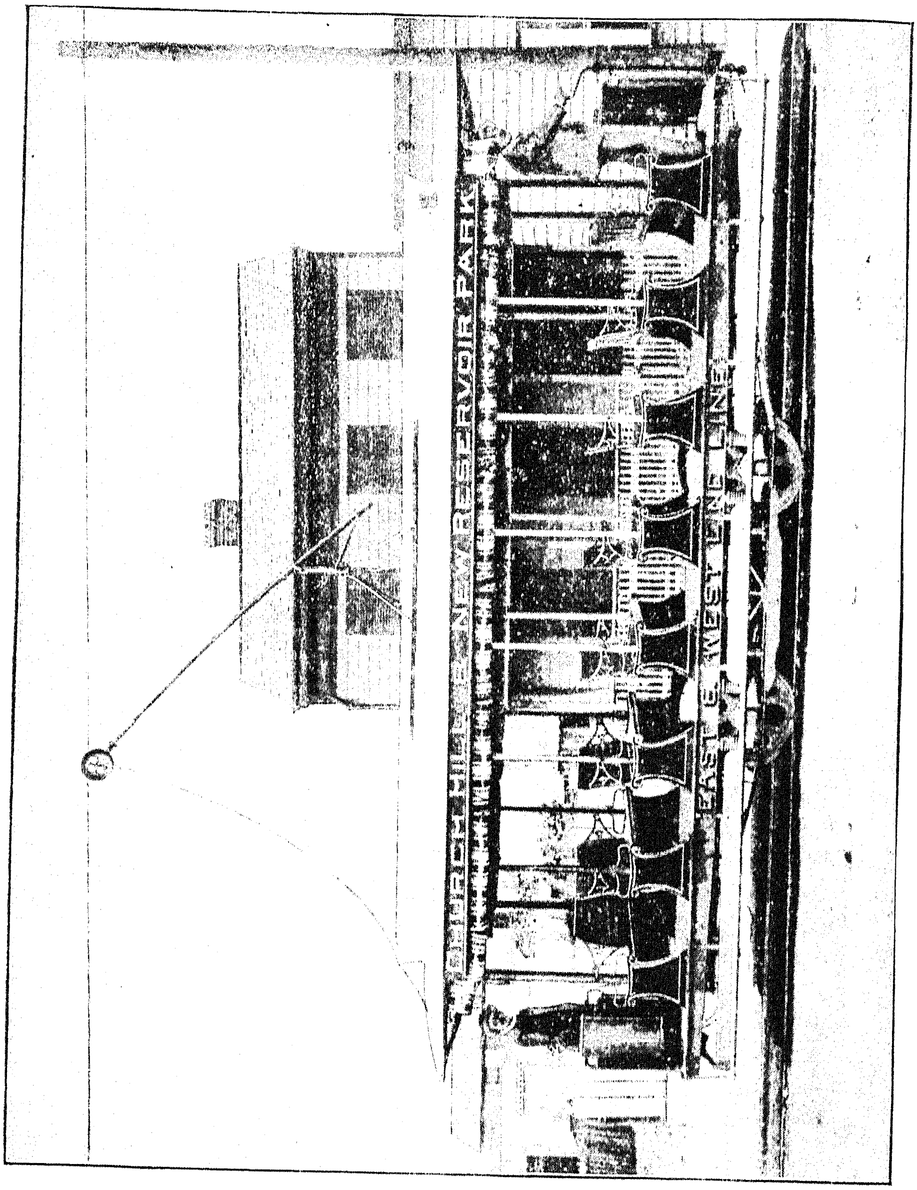


FIG. 6.—OPEN CAR ON RICHMOND, VA. RAILWAY.

which effects the various commutations and reversals. They run with fixed brushes in both directions and under all loads. They are run without any covering, and I have no doubt of having street car motors in such condition soon that the cars will be run over a pit, and the machines washed down with a hose.

The cars can be run at widly different speeds, varying from the slowest crawl to twelve or more miles per hour. They can be started and stopped without the use of brakes in the space of three or four inches, and when making the normal running speed can, in an emergency, be stopped and reversed without brakes within less than a quarter of a car length. The starting, although prompt, is easy and without shock or jar.

Emergencies sometimes bring out qualities of motors which are unexpected, or which at least if counted on are rarely ever brought into play. On one occasion a man was descending a six per cent. grade with a heavy load when his brake chain parted. Seeing a sharp curve right ahead of him which he knew he would leave if he struck it at the speed he was running, he instantly reversed his machines and brought his car to a standstill, and on the rest of his trip handled his machines entirely with the motors. On another occasion a man passing from behind one car directly in front of another was struck and knocked down by the dash-board. Although the car was moving at a lively gait, the machines were reversed and the wheels stopped within a foot of the man. By any other method of propulsion he would have been killed. Frequently on a ten per cent. grade, with a loaded car, the brakes have been thrown off, the machines reversed, the car stopped and started backward up hill within less than one-half of its length, a test of the most remarkable severity.

In noting the control of the car, one is struck at the remarkable character of the traction, and cannot help wondering where it comes from; unquestionably it is in a large measure due to the fact that every pound of weight is available for traction, and that the two axles being independently driven, can take their own speed, and each pair get the best grip.

Another thing to be noted, where the ground circuit is used, is the increase of traction due to the passage of the current from the wheels into the rails. Exactly what molecular action takes place has not been ascertained; it may be simply heating at the contact surface, but the fact remains that the traction is very largely in-

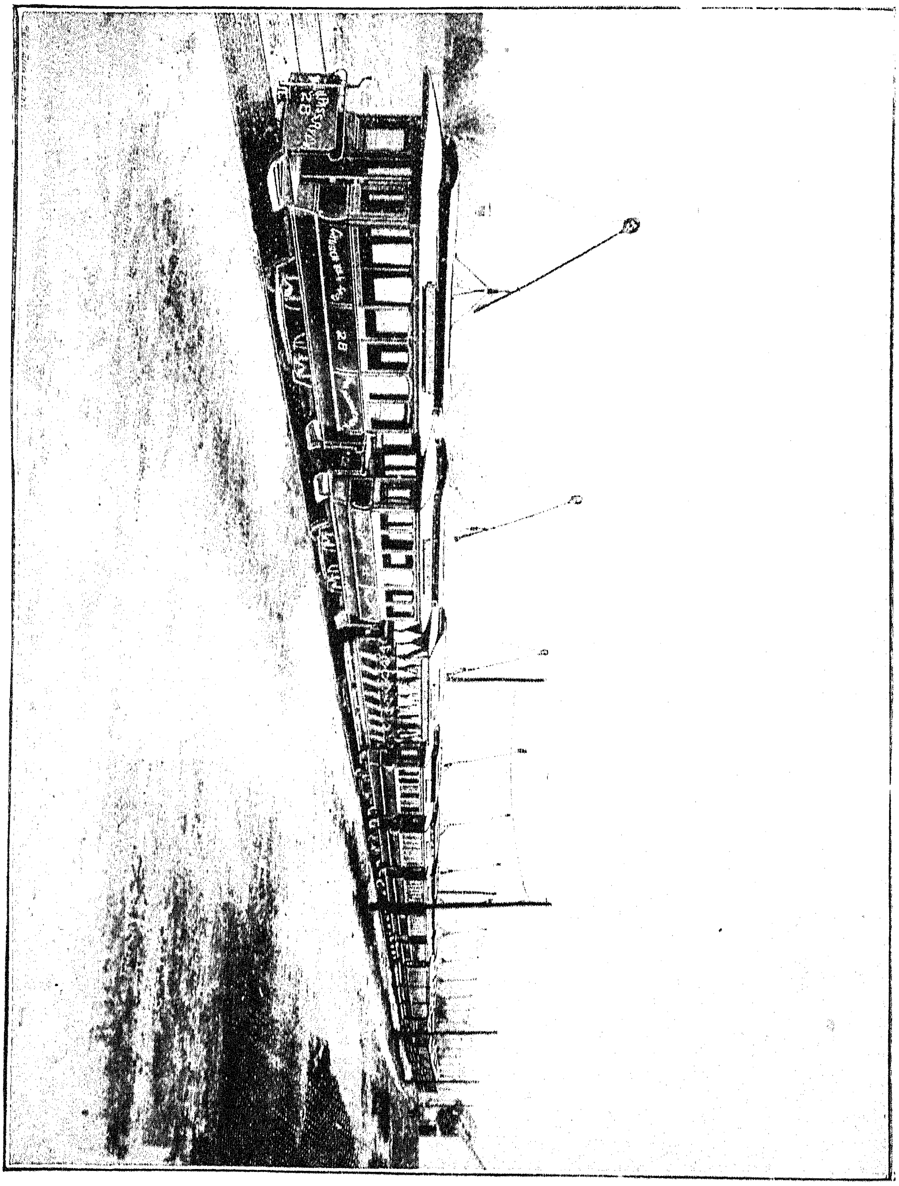


FIG. 7.—OPERATING WITH ALL CARS "BUNCHED" AT EXTREME END OF ROAD.

creased. The magnetic condition of the wheels and tracks also has some function in this increase of traction. The wheels and axles are magnetized by the method of motor construction and mounting, and also by the passage of the current through them; the tracks are likewise magnetized by the current passing through them and also by the rolling contact of the wheels. Yet, admitting all these aids, the results have been extraordinary.

Sixteen-foot closed cars carrying picked loads of fifty-five to sixty passengers, and having a total weight of 15,000 pounds, are operated on the 10 per cent. grade, often with a slippery and slimy rail, and on 27-foot curves, without the use of sand or extra help.

These same grades and curves have been ascended without sand after one of the worst sleet storms which has visited this section for years. Various tests under the most unfavorable conditions have been made, cars often being required to cut their way through clay, snow and ice when the tracks were out of sight. No severer test could have been made, and the power and tractive effort developed have exceeded the most sanguine expectation.

Cars have often been stopped and started on the heaviest grades, and in the sharpest curves. One motor has propelled two cars around sharp curves and up six per cent. grades. One car has pushed another of greater weight around the sharpest curves and up seven and eight per cent. grades both straight and combined.

I have no hesitation in saying that with a clean track, and with proper gearing, these cars could mount a 15 per cent. grade, depending on adhesion alone. During the four months operation, although there have been troubles and changes have been made while the cars were running commercially, no car has been pulled a foot by animal power; if any accident has occurred to a motor the car has gone along with one machine, oftentimes with heavy loads and on heavy grades, or in some cases has gone home in company with another car. One hundred and forty men who did not know the difference between a motor and a hand-saw, have been broken in, to handling these cars; oftentimes with remarkably short training. Up to Saturday, May 4, the number of regular cars on duty had not exceeded twenty. On that day at 4 P. M., the hour when traffic becomes exceptionally heavy, we began putting out ten open cars, starting them from the eastern car-

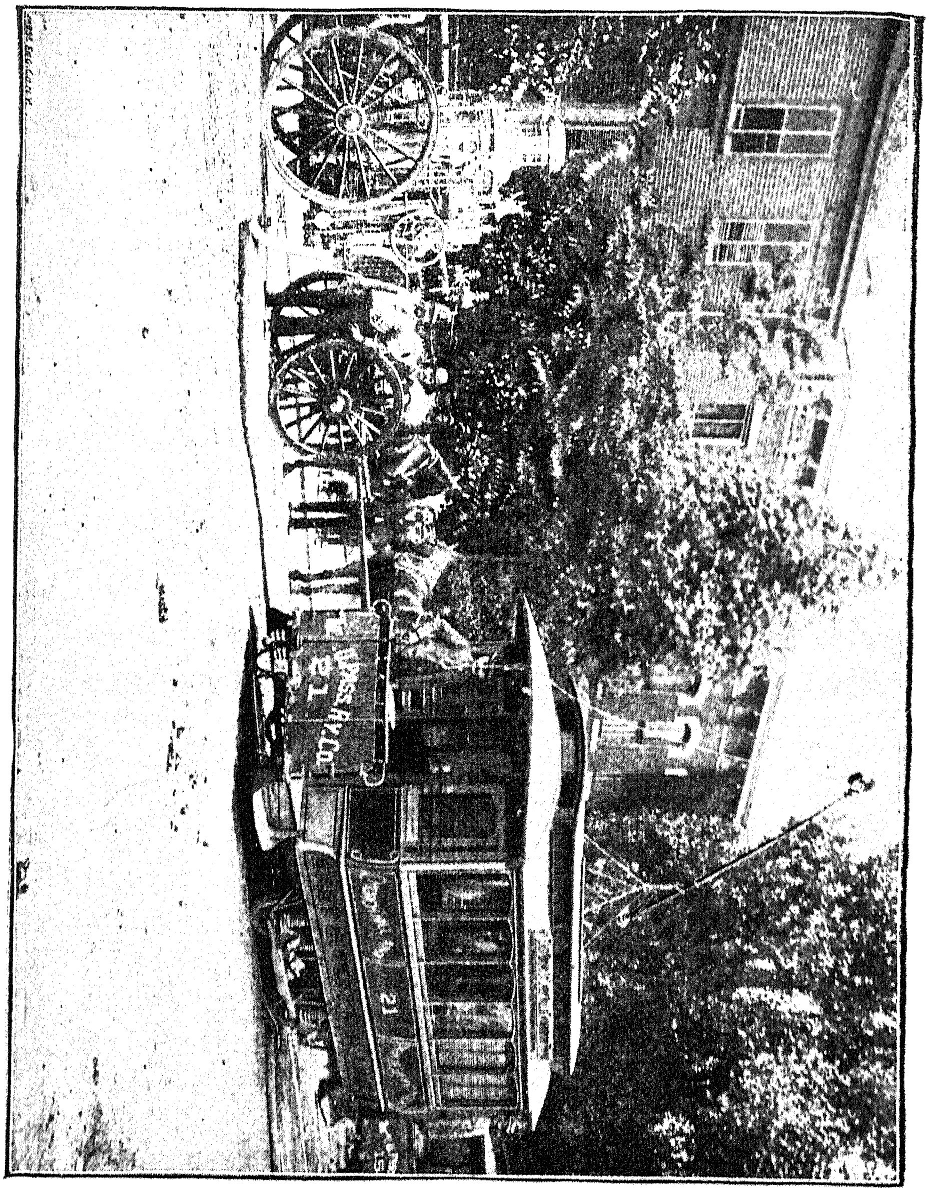


FIG. 8. METHOD OF JUMPING FIRE HOSE ON TRACK.

house alternating with the closed cars, but finally banking them all up at the Old Reservoir. At 6 P. M. they were started home, and shortly afterwards these ten open cars, headed by a closed car, were all run at full speed close together down the east-bound track of Clay street. At one time fourteen cars were on the line between Clay and Hancock streets and the Old Reservoir, and shortly before 7 o'clock there were about twenty-two cars on the line between Capitol Square and the Old Reservoir, and at 7 o'clock thirty cars were on the line, the closed cars being very heavily loaded, many of the cars being run with two minute intervals. The three boilers, whose capacity is 375 h. p., were then supplying three Armington and Sims engines of 125 h. p. capacity, which were driving the six railroad dynamos, and two Westinghouse engines of 110 h. p. capacity, which were driving two alternating current dynamos and exciters, with an equivalent load of 1,060 incandescent lamps. The mixture was enriched and the firing of boilers became pretty active, there being in addition to the thirty cars badly banked, about 110 h. p. taken for lighting.

The highest reading, however, on the line was less than three-quarters of the normal capacity of the dynamos, which were originally intended for only thirty cars, and the average reading for this number was only a trifle over half the normal capacity. Some idea of the effective work of the automatic steam regulator can be formed by an inspection of the steam diagram for May 4 and 5.

The ampère readings taken at intervals of ten and fifteen minutes give a load diagram which is really higher than a continuously recording instrument would show.

The figures which follow, being based upon an actual service increasing from eight to thirty cars, and extending over a period of four months under the most trying circumstances, and at the most critical and care-requiring period of the enterprise's existence, will give street railway men the most careful and liberal estimate of every possible expense attending the operation of the road which is in any way dependent upon its electrical features. The operating expenses are given under two heads; first, the central station operating expenses, and second, the road operating expenses, the two constituting the total cost of motive power. These are given in detail, and they will probably be found to cover every expense, although in some small ways the distribution of these expenses may be found to be changed by several months longer running. The fuel and other incidental expenses of the station are from the engineers' record, and checked by the readings of the electrical instruments.

For thirty cars running on a double track the central station expenses are about as follows:

58 42-100 cwt. coal, at 10 cents	$5.84	
113 70-100 cwt. screenings, at 7 cents	7.96	
5 pints cylinder oil, at 75 cents per gal	.47	
5 pints dynamo oil, at 30 cents per gal	.18	
3 pounds of waste, at 10 cents	.30	
Water at current rates	3.00	
Fuel, etc		$17.75
1 day engineer	$3.00	
1 night engineer	2.25	
4 firemen	6.00	
1 cleaner	1.50	
2 dynamo men	3.50	
1 electrician	4.00	
Labor		$20.25
Lighting		.50
Depreciation and repairs on dynamos, 3 per cent		1.50
Depreciation and repairs on engine and boilers 10 per cent		4.38
Total central station expenses		$44.38

Or $1.48 per car making 80 miles.

[The central station being operated for twenty-four hours for lighting and power purposes, the lighting company has now assumed charge and rents the power to the cars at $1.60 per day.]

In estimating the road expenses more than liberal allowance is made, but in a new enterprise it is safe to err on this side rather than attempt to claim for a system advantages which will not in practice be borne out. The expenses are then as follows:

Oil, grease and waste	$4.50	
Brushes	3.00	
Trolleys	2.00	
Material		$9.50

Three mechanics and one helper for thoroughly over-

hauling motors once a week, making repairs, adjusting brakes, etc.	7.90	
Four inspectors, two day and two night, for oiling machines and looking after brushes	7.50	
Two night inspectors to thoroughly inspect motors after cars are laid up and see that all bolts, nuts and connections are tight and that everything is in good working order for the morning run	4.00	
One winder and helper for making any repairs which may be necessary and any testing which is required	3.50	
Two linemen	5.00	
Labor		27.90
Depreciation on car work at 15 per cent	18.00	
Depreciation on line work at 10 per cent	4.00	
Depreciation		22.00
Total road operating expenses		$59.40

or $1.98 per car making 80 miles.

The total motive power expenses both in the central station and out on the road would not then exceed $3.46 per car making 80 miles, or 4.32 cents per car mile, and this includes everything except the executive and the salary expenses, taxes and insurance, and is less than 40 per cent. of the cost of operation by horses with the same number of cars, and under the same conditions.

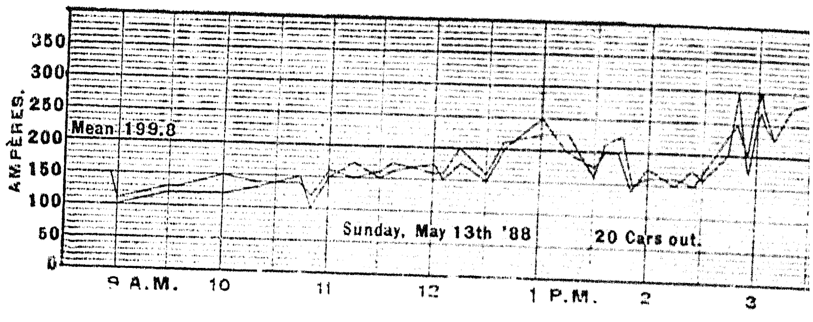
But the showing is even better than this, for another fact is to be noticed, and it is an important one from an economical point of view. For example, twenty cars on duty for about eighteen hours make a mileage of from 1,500 to 1,800 per day, and if it were not for the fact that the schedule requires them to operate part of the time on a single track with insufficient turn-outs they could make 1,800 or 1,900 steadily. This, notwithstanding the fact that the speed is limited by municipal regulations. Many of the cars often run 90 to 100 miles. The same mileage and intervals made by twenty electric cars could not, under the circumstances, be made by anything less than twenty-five horse cars of the same size, and the electric cars probably carry in the aggregate just as many passengers. It follows that to get the same service with horses not only would the number ef cars have to be increased, but likewise the number of horses, conductors and drivers. The same mileage performed by horses without these cars and grades would require a stable equipment of not less than 300 horses.

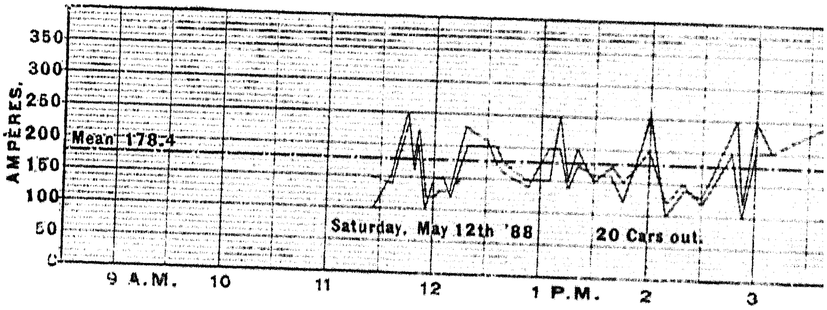
A good idea of the operation of the system may be obtained from the following weekly table, taken from the records of the railway company up to the present date:

Week ending	No. of cars put out for regular service.	Avg. No. of cars out per day.	Miles per week.	Avg. miles per day.	Avg. miles per car per day.	Passengers per week.	Avg. passengers per day.
Feb. 8, ...	72	10.3	2,632	376	36.5	18,764	2,680
15,....	48	6.9	1,211	173	25.2	6,688	956
22,....	77	1.0	2,604	372	33.6	16,504	2,358
29,....	91	13.0	4,039	577	44.4	22,944	3,135
Mar. 7,....	94	13.7	5,047	721	52.6	25,063	3,580
14,....	86	12.3	5,313	759	61.8	20,897	2,985
21,....	99	14.1	6,916	988	69.9	30,357	4.337
28,....	113	16.1	8,621	1,232	76.3	27,076	3,868
April 4,....	118	16.9	8,351	1,193	70.8	28,260	4,037
11,....	133	19.0	10,089	1,441	75.9	36,683	5,240
18,....	133	19.0	10,583	1,512	79.6	40,090	5,727
25,....	135	19.3	10,895	1,557	80.7	39,040	5,577
May 2,....	137	19.6	11,309	1,616	82.4	45,350	6,479
9,....	140	20.0	10,832	1,548	77.4	51,648	7,378
16,....	176	25.1	11,563	1,652	65.8	58,598	8,371
23,....	186	26.6	12,128	1,733	65.2	55,163	7,880
30,....	194	27.7	12,789	1,830	66.1	57,519	8,217
June 5,....	189	27.0	14.154	2,022	74.9	52,820	7,546
13,....	187	26.7	14,649	2,093	78.4	59,219	8,459
20,....	181	25.9	14,371	3,053	76.2	70,065	10,138
	...	....	178,096	.....	...	762,748	......

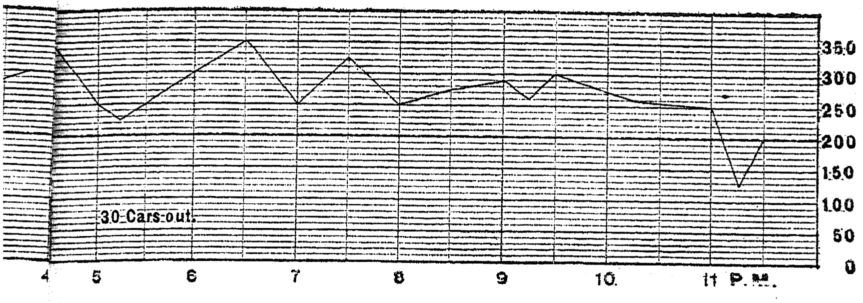
With the mileage which is now made, there is an actual saving of about $125 per day in the motive power expenses as against operation by horses. It is hardly necessary to point out that this would pay a good interest on a pretty large investment. The capacity of the road to economically carry passengers has been made so apparent that the old city railway, which was operated by mules and has about 13 miles of track and 50 cars, has been bought up and is to be equipped with motors operated from the same central station. A third line will probably be added, and in the near future every street car, to the number of one hundred, in Richmond and Manchester, will be electrically operated from one station.

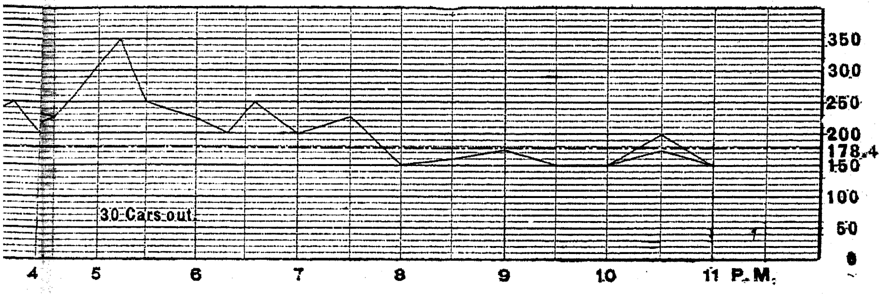
From this same station is operated a power and light circuit.





FIGS. 9 AND 10.—AMPERE DIAGRAMS





WI TWENTY AND THIRTY CARS ON TRACK.

Having thus seen what is possible on a street railway, let us briefly consider the application of electricity on a larger scale.

The failure of the elevated railroads in this city to meet the issue of rapid transit is in no way more forcibly illustrated than by the number of plans which have been brought forward for discussion. No less than eleven have presented themselves, several of which have features in common and claim similar routes; and I will but very briefly review them.

The Beach Pneumatic was merged into the Broadway Underground, and this in turn into the New York Arcade Railway Company, and it is now familiarly known as the Arcade. They propose to run from the Battery, thence to Broadway and Madison Square, with four tracks, the two centre being express tracks, with stations one mile apart, and the two outer tracks for way traffic. Thence the route extends to Fifty-ninth street by Broadway, and to the Harlem River by Madison avenue, with an additional route up the west side of Central Park by the Boulevard or some other avenue, and into the annexed districts. Branches are proposed to connect Broadway and Madison avenue on Twenty-third street, and the two-branch roads on Forty-second street.

The Underground Railway Construction Company has pooled the issues of the New York District, New York Underground and the Metropolitan Junction Company. The route of the first is similar to that of the Arcade and runs from the Battery to Broadway and Madison Square with four tracks; thence up Broadway to Fifty-ninth street, and up Madison avenue over the Harlem River to some point near Jerome Park. Branches are also proposed from river to river on Twelfth and Twenty-third streets. The New York Underground road proposes to run from the City Hall Park east to Broadway, through Centre and Mulberry streets to Lafayette place and Fourth avenue, at Union Square. Thence by two lines to the Grand Central, and Broadway and Fifty-ninth street, up the Boulevard to Kings Bridge, the main line continuing under Madison avenue to the Harlem River and by a loop to Kings Bridge.

The Metropolitan Junction Company proposes to connect with the two last named in the annexed districts, and then run over the Hudson by a bridge at Washington Heights, connecting with a tunnel through the palisades.

The City Railway Company proposes to begin at South and Whitehall streets, crossing Broadway below Rector, running between Church street and College Place, Sixth and Seventh avenues to Fifty-ninth street and the Boulevard. The line would then run up the west side of Central Park, crossing the Harlem River above High Bridge, terminating in New Rochelle. Branches are proposed for the principal ferries on both rivers, the Post Office and Grand Central Station. The roadway is intended to be 50 feet wide and 12 feet below the street surface; it is to have four tracks, two way and two express. It differs from the other underground railways in that instead of taking the street lines it proposes to purchase the necessary property and erect buildings over the line of the road, which are to be rented for sufficient to pay the bonds of the company. The road thus running through the basements, so to speak, of the buildings, and crossing the streets by tunnels, becomes a covered roadway instead of an open one.

The Terminal Underground Railway Company proposes a four-track underground railway from the City Hall along the proposed Elm street improvement to the Grand Central Depot.

The Metropolitan Transit Company proposes two elevated steel structures, one on Broadway, between Chambers street and Forty-third street, connecting with the Grand Central Depot, and the other on the West side following the line of the water front to Gansevoort Market, thence up Tenth avenue and the Boulevard to Kingsbridge.

The plan proposed by Heman Clark is one of the most radical. He proposes a four-track tunnel, beginning near Fleetwood Park, cut in solid rock 150 feet below the surface, coming down about 100 feet west of the line of Broadway to the Astor House, where it would connect with two two-track branches, one going under the North River to Jersey City and Newark, and the other under the East River to Brooklyn and Prospect Park. Passenger stations would be at the foot of shafts sunk every two miles, and freight stations at intermediate shafts where freight pockets would be tunneled. Large elevators would be used for handling the passengers and freight.

All of these schemes have their good and bad points which I do not propose to here discuss; but whichever is successful, they hope to use electric motors for propulsion. Now whatever we claim for electrical motors, it would be folly to attempt the handling of the traffic of such a road without full knowledge as to the character of the work which has to be performed and a perfectly definite idea of the system of train dispatching which will best meet the wants of the public, and, at the same time, preserve an effective and economical system of operation. To fully consider this let us briefly review the work which is being done on the elevated railroads, for example on the Third avenue line.

This is eight and a half miles long. Grades vary from eight to one hundred and five feet to the mile. The level stretches amount to about one-third of the whole distance, and this includes the stations. On the seventeen miles of single track there are fifty-two stations. In the busy hours there are no less than sixty-three four and five-car trains on the tracks. These trains weighing from eighty to ninety-five tons, make the half trip in forty-two minutes, including stopping at twenty-six stations and discharging and taking on passengers, or at the rate of about ten traffic miles per hour. The work of the engines, which may be divided into three parts, overcoming the train's inertia, lifting the train on the grades, and traction, and the maximum is at least seven times that necessary for traction at mean speed on a level. Three times in every mile this weight of 80 to 95 tons must be started from a dead rest, raised to a speed of twenty or more miles an hour and brought to rest in about eighty seconds. The engines have to be run with 130 pounds boiler pressure, and have a capacity of 185 h. p. Fifty-nine per cent. of the power on a round trip is used in starting, 24 per cent. in lifting, and 17 per cent. in traction, and the average power developed per minute per round trip, including stoppages, is 70.3 h. p.

The traction necessary to handle these trains is obtained by a locomotive weighing 22½ tons, of which only 15 are carried by drivers five feet apart. With an engine and the forward trucks of one car between two girders, we have a weight of nearly 1,500 pounds per running foot. The present weight of engines is not sufficient to properly handle these trains when the tracks are in a slippery condition, and it is not wise to increase it. It is absolutely necessary to keep up the high character of the engineering work of this road, and every consideration of business prudence and public safety will permit of no laxity in this direction. Yet these locomotives have done wonderful work. Although on duty for twenty hours, with steam admitted to the cylinder for six hours only, with an average horse-power of only 38 per cent. of the maximum, and with all the incidental losses of steam, they develop a horse-power with less than a total expenditure of 6.2 pounds of coal per hour; and the coal expense alone is only about $1.15 per round trip, or 2 $^1/_5$ cents per train station. The operating expenses are 62 cents per train mile; or, allowing an average of four cars per train, 15½ cents per car mile; and this includes everything.

The percentage of expenses of operation and maintenance for the whole Manhattan system may be roughly expressed as follows:

Motive power....	42.8
Maintenance of cars	5.4
Movement of traffic	38.4
Maintenance of way	8.2
General expenses	7.2
Total	100.0

The coal costs over $700,000 per annum, which is more than the wages of the engineers and firemen, but it is only about one-sixth of the total operating expenses. Hence there are other reasons than merely a saving in coal which must be urged in favor of an electric system as against a steam system.

Generally speaking, the real needs of rapid transit are not best subserved by the handling of long trains from a single tractive engine. True, the conditions of service on the elevated railroad, the crowded condition of the track, and the great cost of operating independent steam engines makes it impracticable to work with small train units there. But the fact remains that the effective length of train which can be operated by any system is limited; that this limit has already been reached on the elevated railroad, where only one-sixth of the weight is now effective for traction; and that the smaller the units the less the momentum of the train, and the easier it is to quickly and efficiently handle it and the less will be the liability of, and danger from, any derangement of the brake apparatus. Furthermore, the thrust of the moving weights on the grades, the vibration due to the shifting of the weight along the girders, the tensile, shearing and crushing strains on the structure are all materially increased by the use of a heavy locomotive at the head of a long train.

But since we must endeavor to meet all conditions of engineering, we must try to meet that one which requires the operation of a heavy train by a single motor car. In this case I deem it advisable to make the motor car of about the same size as the cars which form the train, equip both trucks and thus spread the weight used for tractive purposes over a larger distance. We have now partly constructed a 300 h. p. motor equipment of this character.

This car, whose general external dimensions are similar to those of the regular elevated passenger coach, differs in its construction in many important details. The front and rear platforms are inclosed, and about ten feet at each end of the car—that is, the space over the trucks—is partitioned off and reserved for the use of the motor man. The middle space has side entrances, and is fitted for passenger accommodation. At each end of the car the floor framing is raised about twelve inches for a distance of about ten feet from the end, and the car body is carried on two very strong and heavy special iron trucks mounted on forty-two-inch wheels fitted with five-inch steel axles. Each axle has an independent movement and is driven by a tandem motor, centred upon the axle, which bears a part of the weight. The motor is also carried by heavy compression springs hung from the truck, by means of which as much of the weight of the motor as is desired can be thrown on to the boxes. Each motor, which is about 75 h. p. normal capacity, has two armatures, each of which is connected to a fixed and adjustable gear on the axle, the ratio of the gearing being about 1 to 3. These motors will have one brush on each commutator, and that will be a top brush. Each motor has directly over it, in the engineer's room, trap-doors fitted with heavy glass panels, which allow the machines to be seen at all times, and through which the machines can always be reached. The controlling devices and switches are all duplicates, and the car can be handled from either end. The car and the trucks are completed; the motors are well under way. The car will be capable of exerting a tractive strain of at least 20,000 pounds—more than double that of the present locomotives.

But that is not the ideal method of traction.

By a system of electrical propulsion, the power can be distributed underneath the cars, every car, or every two cars if need be, being a unit; and, although I do not think it advisable, arrangements could be made for propelling a large number of cars under simultaneous control. By distributing the power under the car, the whole weight of the car and passengers can be made effective for traction. This will enable the cars to be started more promptly, brought to speed more quickly, and stopped in shorter intervals, increasing the mean rate of speed for a given maximum, or vice versa, reducing the maximum for a given mean rate. By making cars individual units of locomotion, the intervals between trains may be made one-fourth or one-fifth of the schedule which must exist where four or five-car trains are used. It may be said that this will increase the number of employés that are necessary. This is true, but it will increase the number of passengers who, both day and night, would make use of the system, in such a ratio as would make this increase of expense insignificant.

Let us consider a possible system which would go a long ways towards solving the question of rapid transit. I will briefly point out its features. It would have four tracks extending from the Battery up Broadway to Twenty-third Street; thence diverging in two divisions, each still with four tracks, one along the line of Madison Avenue to the Harlem River and terminating near the Jerome Park district; the other following Broadway and running up the line of the Boulevard and Tenth or Eleventh Avenues. Two tracks would be reserved for express traffic, with stations a mile apart, and two for way traffic, with stations every third of a mile, every third way station corresponding with an express station. The tracks would be arranged in two tiers, the express tracks being the lower. The entire system would be lighted, ventilated and operated by electricity, to be supplied from four stations on the water front, one at the Battery, one at East Twenty-third street, one at Madison avenue crossing of the Harlem River, and one on West One Hundred and Twenth-fifth street; the stations to be operated by compound condensing engines in three units connecting directly to large, slow moving multipolar dynamos, two engines being of sufficient capacity to handle the demand made upon any station; trains to be operated with one, or at most two car units; motors to be on the trucks of one car, centred on the axle and with one reduction of gearing; express speed to be thirty miles an hour, and way trains to make twelve traffic miles per hour; the current to be delivered to the cars by an overhead system with underneath contact, the contact wheel to be carried on the car, supported over the centre of the truck and trending a little abaft it. Freight pockets at the level of the way track could be run under the cross streets at express stations. Track to follow a line of grade of not more than 25 per cent.; access to stations to be by stairways, and where there is any material difference between the street level and the track, by hydraulic elevators, operated from constant pressure tanks kept supplied automatically by motors. Freight trains to be run on way tracks.

Supplementing this system should be a line of surface cars, likewise operated by electricity from the same general source. In the heart of the city these can be operated from a conduit, or by the storage battery, and in all the suburban extensions by the overhead line system.

The objection of loss in transmission and recovery has been raised as against an electric system, and also the question of braking. These are easily met.

We have systems of electrical braking a train by the use of its motors, which consist in converting them into dynamos, deriving their driving power from the train, and using up its energy by converting it into electrical energy, which can be used in a variety of ways. In a storage battery system the energy of the car can be made to partially recharge the batteries on a down grade, and in stopping. In a direct supply multiple-arc system, operating a large number of cars, this power of being able at will to convert the energy of a moving train into electrical energy delivered to the line by the same apparatus which controls the speed and power of the train, is of exceptionable importance. In a double track system, the energy given to one track may be communicated to the other just as efficiently as along itself. In fact in the entire electrical system every part is mutually connected, and there is a bond of union between the different trains far more perfect and effective than any mechanical connection could be. The up-grades on one track being equal to the down-grades of the other, it is evident that the total up-grade of a system is equalized by the down-grade thereof. If, then, energy is expended in getting under way and climbing up-grades, and a large proportion of this can be recovered in stopping and in running down grade, it is evident that the total initial energy required would be that necessary to remove the trains continuously on level grades, with a percentage for loss in conversion and reconversion added. The more train units which are operated, and the more frequent the stoppages, the more marked is this advantage in an electric system.

I some time since made a number of determinations of a system of the equipment capacity of the Third Avenue Elevated, in which I found that about 40 per cent. of the power could be saved by this system of braking, where every motor coming to a station or running on a down-grade is made a generator which is helping to supply the current needed to operate the remaining trains.

Instead of the current being all supplied by a main generating station at one or two points, it would be supplied from nearly as many moving stations along the line as there would be trains slowing down or running on a down-grade. In fact, the loss of the two conversions and one transmission would be more than counter-balanced, and consequently the original horse-power at the central station would be no more than the aggregate net horse-power now developed on the entire line, and this is only about 38 per cent. of the total capacity of the engines. With any given potential and losses on the line, the main conductor would be very much smaller. Not only on the line, but at the central station would a saving be affected; for in a system of the capacity of that which we are considering this saving on lots, buildings, boilers, engines, dynamos and fittings would be not less than $400,000, and there would be about 40 per cent. saving in the coal, labor, deprecation and incidental expenses of the central station.

Another method of using the motors for braking purposes is to break the connection from the line and reverse the machine through a local circuit on the train, varying either the local circuit or the circuits of the machine. While in this position of braking, if the local circuit is opened and the line connection made the machine would be instantly reversed.

Elaborate experiments were made by me nearly two years ago on the Thirty-fourth street branch of the New York Elevated Railroad, in which these questions of gearing, of mounting the motors on trucks, under the car and handling the car without shoe-brakes were very thoroughly studied. There this method of braking was shown to be under the most perfect control. When desired, the braking could be made so sudden as to cause the wheels to have a continuous skidding rotation, not such a skidding as is caused when an air brake is put on too sharply, but a rotating slip which is just enough to relieve the armature when the strain on it reaches the point determined by the co-efficient of adhesion on the rails.

This is the ideal method of braking, because fixed skidding and flat wheels are an impossibility, and the wheels will turn until the train comes to a dead stop, although, where the braking power is put on too suddenly and exceeds the grip of the wheels, they will relieve themselves by slipping just enough to keep the braking at the maximum limit. A train so governed can be made to creep down the maximum grade of a road at a snail's pace, and in an emergency such a car, running 21 miles an hour on a down grade, has been stopped and reversed within 90 feet.

When an electric motor is reversed its action is cumulative, for so long as its motion continues in its normal direction, it tends to augment the current which flows through it with any given setting of the switch, and thus very much increases the effort to stop itself; and the higher the velocity at which the armature is traveling, the greater this effort. In fact, it would be practically impossible with a properly constructed railroad motor to prevent its reversing when the switch is thrown over far enough. This is another point in which an electric motor has a great advantage over a steam engine.

So it would seem that electricity has practical advantages over steam for this character of traffic, and there are considerations other than purely engineering ones. The use of electricity instead of steam would do much towards making a road of this character popular. We would have express and way trains, starting and stopping smoothly and quickly, moving at a high rate of speed and running at short intervals. Dust, smoke, cinders, oil and drippings would disappear; the air would be purer and cleaner. The train would be safer and under more perfect control; motive power would cost less; depreciation on the track structure would be much less; the carrying capacity would be increased; cars would be brilliantly lighted; comfortably heated; in short, everything which would make rapid transit safe, pleasant and effective is possible by a properly developed system of electric traction.

One of the most important sources of revenue from such a system would be the sale of power for stationary purposes. Already we are operating no less than 130 different industries by electric motors, and the receipts vary from $60 to $150 per horse-power. The extent of the application of electricity with any such general source of supply can hardly be conceived. Its profits would be far more than enough to pay the total cost of motive power.

But we must not shut our eyes to the work which has got to be done. We must not exaggerate the advantages of electricity; we must not belittle the position of a system which has been developed by the ablest engineers for the last forty years. All their experience, all their study, all their victories form a magnificent groundwork for us to build upon, and will render the final victory of electricity more easy and certain if we build wisely and take to heart all the lessons which their years of experience teach us. In a small fraction of the forty years which have marked the development of steam engineering, electric engines will rival, in mechanical perfection, in construction and performance, the best locomotives. Even to-day, weight for weight, and duty for duty, the electric motor is already rivaling and outstripping its predecessor. For the present and for a long time to come it is probable that for trunk line work, and for the original source of our power in an electrical system, we will be dependent upon the steam engine, but we all hope and expect that the future will bring us some other effective means of converting the energy of coal into electrical energy, and that by some direct method which will not require the intervention of a steam engine and dynamo. It is not too much to hope for, and already experiments have been made which foreshadow this great result. The success of this discovery would produce such a revolution in the use and distribution of power as to exceed our most sanguine expectations.

THE PRESIDENT:—We have all listened with great pleasure and profit to the very able, interesting, and instructive paper of Mr. Sprague's, and as the hour is now late, I suppose we cannot enter into any extended discussion upon it to-night. I think it would be very proper to pass a vote of thanks to Mr. Sprague for the pains and care he has taken in presenting these matters to us, and furthermore for the extreme freedom with which he has spoken of his difficulties. It is a great indication of a growing art when electrical people can get together and discuss questions in which they are interested. For many years past there has been a feeling of seclusiveness among electrical engineers, which has been unfortunate. Such papers as we are having presented to us from time to time, indicate that that period has passed. The subject of electrical propulsion, as you are all well aware, has received much attention during the past few years. Its rapid growth is astonishing, when compared with the growth of steam engineering. It is really very marvelous indeed. We have had presented to us to-night, a general scheme of electric railroads as applied to Richmond, Va. Those who know some of the difficulties that Mr. Sprague has had to encounter in this undertaking, cannot refrain from complimenting him most highly, first, upon his courage and perseverance and, second, upon his skill in overcoming the difficulties. (Applause.) The difficulties are much more serious than they appear to us on paper, and I can fully appreciate what it is to deal with such a problem. That Mr. Sprague has met with such success, and has actually received his pay for a railroad of that magnitude, running 30 cars, is I think a strong indication of what we may expect in the future.

Upon motion, a vote of thanks was then unanimously rendered to Mr. Sprague.

Mr. Sprague:—I regret the time is so limited as to prevent a general discussion of the paper this evening, as I fully realize that the suggestions which others may make are oftentimes the highest possible aid. I would not be just if I should accept to myself the kind remarks which President Weston has made. The success of the Richmond railroad is due to the courage, zeal and constant work, day and night, of the gentlemen who have been associated with me. I have had associated with me Messrs. Green, Crosby and others, who have been on the ground, and who have done everything to strengthen my efforts when I was not able to give attention to the business myself. Of course we have made mistakes, and will probably make a great many more, and if time permitted this evening, I would have been happy to have gone into detail on the difficulties which I met with. Sometimes it is like a nightmare when I think of it; but they were difficulties which we were obliged to meet and overcome, difficulties which could not be foretold, and which had to be met as they came up. I thank the Institute again for their kindness. (Applause.)

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

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Special Meeting.

The twenty-eighth meeting of the Institute was held at the House of the American Society of Civil Engineers, No. 127 East 23d Street, on Tuesday, October 9th, 1888.

The meeting was called to order at 8 P. M. by Secretary Ralph W. Pope.

The Secretary:—In the absence of the President, the indisposition of one of the Vice-Presidents and the objection of another one to serve this evening, it will be incumbent upon you to select a Chairman, and I am ready to receive nominations for a Chairman, *pro tem* for the evening.

Upon motion of Mr. Wetzler it was voted that Mr. George M. Phelps act as chairman for the evening, and he took the chair accordingly.

The Chairman:—Gentlemen, I am indebted to you for the compliment of being called on to preside to-night. I will ask the Secretary to state the programme for this evening.

The Secretary.—As most of you are aware, the business of the evening, as stated in the call issued, is the discussion of the paper read by Frank J. Sprague before the Institute on June 19th. On account of the length of the paper it was deemed wise to defer the discussion until this fall, and in considering the advisability of taking it up this evening, the Council decided that as we had a member in Richmond who had taken great interest in the work to which Mr. Sprague referred, and was in an entirely independent and unbiased position, he would be able probably to give us some valuable information in regard to the work done. In addition to Mr. Leonard's contribution to the discussion, I have a note from Mr. Almon Robinson, Lewiston, Maine, which will also be read, in regard to gearing.

Mr. Leonard then read the following:

SOME OBJECTIONS TO THE OVERHEAD CONDUCTOR FOR ELECTRIC RAILWAYS.

BY M. B. LEONARD.

Of all the valuable papers read before the Institute, probably none has excited greater interest than Mr. Sprague's admirable contribution on "Electric Railways, the Solution of the Municipal Rapid Transit Problem," in which the advantages of electricity in practical operation have been so well set forth as to convince the most skeptical, of its superiority over all other methods of traction for this class of traffic. Although the paper was written with special reference to the adaptability of electricity in solving the problem of rapid transit in the great city of New York, the benefits resulting from this system of propulsion may be obtained equally as well elsewhere, and prove to be as permanent as we have found them in our own historic city of Richmond.

To electrical engineers this comparatively new application of the electric current, is one of the most important questions that have come before them, and should be carefully studied in all its details, so that the defects and objections may be pointed out and provided against, and the most perfect methods developed as speedily and fully as possible. But in working this question out, it must be remembered that "the more haste, the less speed," and that before undertaking to surpass the locomotive, it is wiser to prove superiority over the horse or mule. Having accomplished this successfully, then it is time to endeavor to rival steam. In attempting what have been aptly described as "feats of sensational engineering," it should always be borne in mind, that failure inevitably deals a severe blow to the whole cause of electric traction, the general public not being always inclined to discriminate in such cases, as will be remembered from the accident which occurred to a number of capitalists and prominent people some years ago who were engaged in testing the traction system of a well-known electrician.

A second point of the utmost importance, is the necessity for first-class workmanship. Anything worth doing at all is worth doing well, and particularly is this the case with the utilization and development on a large scale of a new force. This is so self-evident as to require no further comment.

While it has been successfully demonstrated in the city of Richmond that electric traction is really the solution of the rapid transit problem, and we have felt in the increased value of real estate, and the growth of our city in population, and reputation, the substantial benefits resulting therefrom, I have considered from my daily observation of the operation of this electric road, that the overhead wire system cannot be made a perfect or ideal method of operation under all circumstances, because of the defects inherent in such a method. But that it will serve as a pioneer in the new field, to be supplanted in time by a more perfect system, I have not the least doubt, and in the following remarks I will endeavor to show some of the objections that have come to my notice, although there may be others that have escaped my attention owing to the extreme reticence of all parties connected with the enterprise, doubtless from business reasons.

Of the three most commonly used, the overhead system appears to be the favorite. The latest report of electric street railways in operation in America, showing that out of a total of forty-three, not less than thirty-six are using the overhead system, and that at least thirty, now under construction, have arranged to do so. Doubtless the smaller cost, as compared with the other two, was an important factor in deciding so largely in favor of the overhead conductor.

As used in Richmond, the details of operation have been shown so fully and clearly in Mr. Sprague's paper, that it is unnecessary to repeat them here, beyond stating that the rail and earth system is adopted in connection with the overhead wire.

During the seven months of active operation of the Richmond Union Passenger Railway, many thousand passengers have been successfully transported every day, and at a very much lower cost than could possibly have been done by horse power, owing to the excessive grades encountered, but the defects of the overhead system are now becoming more apparent as the plant suffers its natural depreciation, and we are now in a better position to judge of its value as a permanent institution than we could possibly have been heretofore.

It is an undisputed fact that the encumbering of the streets with the poles required for the overhead system is one of the most serious objections to its use, although they are hidden to a large extent in many places by the shade trees and foliage. Yet on the main streets of our cities where this system is most likely to be used they are not only a grievous eyesore, but also increase the liability to accident from runaway horses and other causes. Their appearance would not perhaps be so offensive were it possible for them to maintain an erect position, but on account of the weight of the trolley wires, and the pressure of the trolley, the tops of the poles are constantly pulled together, notwithstanding they may have at first been raked in the other direction, and thus the unsightly effect is augmented.

Those of us who are acquainted with telegraph line construction can appreciate the great leverage exerted by a heavy strain on the top of a pole twenty-five or thirty feet high unsupported by guys or otherwise, and will readily understand that it is only a question of time before the poles must be reset or replaced with heavier ones. This has been especially the case on the curves where the strains have been greatest, in some instances approximating a pull of 650 lbs. Nor does the net work of wires; span, trolley, guard and curve, inspire the public with any feeling of toleration for the disfiguring of its streets, but must hasten the time when the clamor for underground wires will compel their removal.

But to my mind the most serious objection is the employment of the bare trolley wire carrying a current of 500 volts and about 90 amperes, which I consider not only dangerous to life on account of the frequent breakages of the wire—which on one occasion last spring in falling struck and killed a horse—but also in damage to property by fire. It is claimed that the use of guard wires will always prevent the possibilities of such an occurrence, but the guard wires did not obviate the burning and melting of the telephone wires when on one occasion the breaking of an exchange telephone pole threw them across the trolley wire, and it was necessary to shut down the railway to prevent the entire destruction of the exchange and possibly the firing of the subscribers' instruments and houses.

That the trolley wire is liable to breakage at anytime has been shown twice during the past week to the interruption of travel and dissatisfaction of the public. While not so very long ago the wires over one of the curves were pulled down on top of the car by the trolley catching in it, and travel entirely suspended on that part of the line for one or two days.

A very serious defect in the use of the bare trolley wire is the difficulty of securing proper insulation. Thus far porcelain knobs seem to have been found best adapted for this purpose, and the leakage resulting, especially in damp or rainy weather must be considerable on the whole length of the line, and must cause a large waste of current. This is probably the reason why it has been impracticable to run over about thirty cars at one time out of the forty with which the road is equipped, and would seem to indicate a very large loss of energy.

In view of the fact that porcelain insulators were long ago condemned for telegraph purposes where a comparatively small current is used, it seems strange that they should have been adopted as the best means for securing insulation in circuits of such high potential as are employed on the Richmond road.

The trolley itself seems to be a great source of trouble, and difficult to manage properly. As has been shown, it consists of a grooved wheel at the end of a counter balanced pole held by a spring against and making contact with, the under side of the working conductor. In rounding curves it must be carefully watched by the car conductor to prevent it leaving the trolley wire and thus stopping the car, and at night extinguishing the lamps which has proved exceedingly disagreeable to the passengers, while on busy days it has seemed necessary to put a third man on each car at additional expense for the sole purpose of attending to the trolley. Sometimes the trolley pole has broken, or the springs holding it against the wire given away, stopping further progress of the car or requiring a man to stand on the car roof holding the trolley pole against the wire until the repair sheds were reached.

One of the most unsatisfactory features about the arrangement of the Richmond road is the ground circuit, not only because of the difficulty of getting a good ground, which has in this case only been accomplished by connecting the track with the whole gas or water works system of the city, to the detriment of our exchange telephone system, so well described by the telephone superintendent, Mr. McCluer, in his paper before the recent telephone convention, but the fact that it is necessary to have a clean track to work on; otherwise vivid flashes of electricity will appear to the terror of the horses in the vicinity and damage to the motor, or should it happen that there is a sufficient quantity of dirt on the rail, it will cause a higher resistance than the potential of the current can overcome, and of course stop the car. Should the motor man get off to push it over the dirt, and fail, as sometimes happens, to switch off the current from the motor, the full current on the line will pass through it just as soon as a good contact is made, and the result is a burnt armature and a disabled car. This seems to have been particularly the case during the recent stormy weather, when I am informed, that the number of available cars for traffic on the road was reduced to about ten. Possibly the saturation of the motor by the rain and the effect of lightning on the trolley wire had also something to do with the case. Owing to the reticence of the employes previously referred to, it has been difficult to ascertain just what damage has been caused by lightning striking the trolley wires and injuring the motors, but that there has been serious trouble from this source, I have no doubt. The very best forms of lightning arresters have not proved sufficient to prevent injury to electric light dynamos where insulated wires are used on the out-door circuits, while the bare wire used for the working conductors on overhead electric traction systems, must always render them more susceptible to trouble from this direction.

After setting forth some of the difficulties which have been shown in the practical operation of the most extensive system of electric traction in the world, it gives me pleasure to direct attention to the remarkable behavior of the Sprague motors under all the conditions to which they have been subjected for the past seven months, which has been indeed remarkable. No load has been too heavy, and no grade too steep for them to ascend. Perhaps the severest test they ever received was on the night of July 4th, in bringing the crowds from the new reservoir park, when the ordinary open cars carried over one hundred passengers, and the closed cars over seventy-five each, so many in fact that the axles of one or two of the cars broke down under the load. All of which must be exceedingly satisfactory to the inventor, and places the Sprague motor at the very head of the many excellent types or forms that have been devised at home and abroad.

The objections referred to apply equally as well to all forms of overhead systems, except where a second wire is used for a return instead of the track and earth, and also where the trolley pole is discarded for the movable trolley. But the increased number of aerial wires required, as well as the heavier fixtures needed to sustain them, render these systems just as objectionable.

We are very anxious to see the effect of ice, sleet and snow upon the operation of our road, as it has not yet passed through a winter in complete running order. A heavy sleet storm has often prostrated our telegraph lines, and I see no reason why the overhead electric railway wires would be exempt from similar trouble, nor why the covering of the rails by ice and snow will not render it difficult to make sufficient contact to operate the line successfully, not to speak of the loss of current by leakage or escape from the trolley wire.

But I have no doubt that the ingenuity of those who have overcome all the problems encountered in opening up with such success the Richmond Union Passenger Railway, will be sufficient to insure its continued operation in spite of wind and weather, and to no one more than myself, will this be gratifying, since it was through my efforts that attention was first directed some three or more years ago, to the splendid opportunities for electric traction that Richmond presented. Although I am free to confess that my inclinations were towards the application of the storage battery, which obviates all the troubles and defects developed by the overhead systems, and against which no more serious objections seem to be brought than the extra weight caused by the batteries and the greater first cost as compared with the aerial systems, all of which, it appears to me, are more than counterbalanced by the difficulties and annoyance in the systems just described.

The very satisfactory results obtained in the operation of the new electric storage cars on the 4th Avenue Railroad in this city, as well as by Mr. Wharton in Philadelphia, M. Huber in Brussels and elsewhere, prove, as I have always believed, the storage battery to be the only ideally perfect system of electric traction for municipal purposes.

THE CHAIRMAN:—Gentlemen, if it is agreeable to you I will ask the Secretary to read Mr. Robinson's paper, as Mr. Robinson, I believe, is not here. Then, after the reading of the papers, the discussion will be open.

There being no objection, the Secretary read Mr. Robinson's communication.

NOTE ON GEARING FOR ELECTRIC RAILWAY MOTORS.

BY ALMON ROBINSON.

Probably no one will be disposed to deny that in devising a connection for the motor and the car wheels, what Mr. Sprague attempted to do, has been done extremely well. Has he attempted all that is desirable?

Mr. Sprague's investigations have shown that, in street car work, a very large percentage of the whole power used is expended in starting the cars. Now it seems to me, that a motor whose speed has a fixed ratio to that of the car wheels, must work uneconomically when the car is moving slowly, for no amount of skill spent in varying the magnetic circuit can prevent an armature from wasting a current sent through it when it is practically standing still.

Perhaps, however, Mr. Sprague may be able to show that a properly designed motor will work with a high efficiency in doing anything which he puts upon it in railroading.

But, even in that case, there would still remain the extra cost and extra weight of a motor large enough to lift its heaviest load by a dead pull.

To do the hardest part of his work, Mr. Sprague is really using a thing in which he disbelieves—a slow moving motor.

In the storage car problem this matter of weight, and bulk, and cost, becomes the most important element. Here, the present practice is simply, the hauling about of ten men to do by main strength what one man could do by a lever.

The desirability of a variable leverage for the motor has not escaped the attention of experimenters. In Mr. Henry's Kansas City work, an arrangement of toothed gearing and clutches was tried. I have never learned whether this particular feature proved a help or a hindrance; nor whether it is in use on the Henry road said to be running at San Diego, Cal. A prominent inventor has expended a good deal of ingenuity on the details of a plan making use of reciprocating friction pawls. Mr. Reckenzaun tried and abandoned a leather covered chain, clamped between two cones in such a way that the diameter of the circle of contact could be varied.

Mr. Van Depoele has suggested, and I presume tried a connection having two different speed ratios, in which friction gears were used. For this particular purpose (furnishing a variable speed ratio) I am obliged to confess myself a believer in the possibilities of frictional gearing.

For electric railroad work, it is certainly under a cloud at present. Those who have used it most, like it least. The eminent engineer, who, about a year ago, read a paper before the Institute on "Methods of Gearing," seemed to consider that it had been sent to its last resting place by the experts who tested some of Prof. Fleeming Jenkin's devices.

No one seems to notice that every day, and on every railroad this method of transmission gets the roughest possible testing; that every locomotive pulls its train by frictional adhesion; that a rail and driving wheel are neither more nor less than a frictional rack and pinion. When we try to copy the work of the locomotive under other conditions our troubles begin. But no man who has ever seen a freight engine climb a grade has any right to speak with disrespect of frictional gearing.

The real root of all the difficulties with frictional transmission seems to be that the pressure producing adhesion does not vary with the dimensions of the gearing; so that when we reduce the size and increase the speed of the gearing, the friction at the journal bearings soon becomes enormous. For light work this is partly obviated by using, instead of metal, some more adhesive material, such as paper, rawhide, etc. But none of these surfaces will stand a pull at all comparable with that on the surface of a locomotive driving wheel.

The most promising methods would appear to be those in which the pressure is prevented from acting across the bearings. This idea is carried out to some extent in the well known V grooved gearing; but the unavoidable sliding and grinding of the surfaces makes it impossible to obtain a high duty from it. The idea is more fully carried out in the devices of Sellers, Jenkin, and others. I do not know that anything has been suggested—I certainly cannot suggest anything—which does not leave, for the purpose under consideration, much to be determined by trial. Knowing well the effect which actual trial is apt to have upon untried devices, I cannot blame an engineer who prefers to let well enough alone, if a fixed speed ratio is all that he cares for. But I cannot help thinking that a usable connection, which leaves the speed of the motor independent of that of the car, would be a valuable acquisition; and that a probability of obtaining it would amply justify further experiments with methods of gearing.

Lewiston, Me., October 3rd, 1888.

THE SECRETARY:—I might say, in connection with this note of Mr. Robinson's, that he was present at the meeting at which Mr. Reckenzaun read his paper, and he told me personally, that he was much interested in the subject of frictional gearing, and he felt that it had received a "black eye" from Mr. Reckenzaun's paper.

The notices which were sent out announced probable contributions to the discussion by Messrs. Mansfield and Blackwell. I have a letter of regret from Mr. Mansfield of his inability to be present, and I am not certain whether Mr. Blackwell is here this evening. There are no other papers.

DISCUSSION.

THE CHAIRMAN:—Is Mr. Blackwell present to-night? If so, we should be very happy to hear from him. He does not seem to be here.

The interesting paper of Mr. Leonard and the note of Mr. Robinson seem to afford very good starting points for the discussion, and the question is now open for any gentlemen who may wish to speak. I see there are a number of gentlemen more or less intimately connected with electric street railways and electric power present, and we should be glad to hear from any of them.

We should be pleased to hear from Mr. Ries of Baltimore, who, I see, is present with us this evening.

MR. ELIAS E. RIES:—Mr. President and Gentlemen:—It is not often that a paper upon so broad a topic contains so much solid meat, within, comparatively speaking, so limited a space, as the one we are here this evening to discuss. Mr. Sprague deserves to be congratulated upon the ability and thoroughness with which he has presented the subject, and I take pleasure in adding what little I can to the discussion of such of its features as it seems to me require more elucidation.

The paper, generally speaking, may be divided into two parts; one dealing purely with the "Municipal Rapid Transit Problem" as determined by the conditions that obtain in New York city, and the other with regard to the construction and operation of ordinary electric street railways. I shall take up the latter phase of the paper first, and then, if time permits, will have a few words to say on the general subject of rapid transit as applied to the requirements of New York city.

Mr. Sprague in his paper refers to several methods of converting the momentum of a train into useful current, one of these consisting in converting the propelling motor or motors of a train into braking generators when running on a down grade or coming to a stop, and returning the current generated back into the line for the purpose of assisting other motors. This method necessitates raising the counter E. M. F. of the motor until it exceeds the direct E. M. F. of the line, and as this latter is 400 or more volts, it will be apparent that only in a few exceptional cases is the train energy or momentum sufficient to develop in the motor or motors anything above the initial electro-motive force for any appreciable length of time. During all this period the energy of the train is simply being expended in overcoming the direct E. M. F., or in other words, in preventing the entrance of the line current to the motor by developing the 400 or more volts necessary to oppose it, a result that can be as readily obtained without the expenditure of this energy by simply opening the motor circuit. The practical effect of the method referred to will be to partially brake the train, but except in a few extreme cases the line current derives no benefit or assistance therefrom. There is, however, another and more economical method of conserving the surplus energy of a railway train, which avoids the loss of the bulk of this energy, and at the same time permits of the utilization of this energy *upon the train itself* at times when it is most required. This method, as developed by me some time ago, consists in the employment of a secondary battery, so arranged as to be connected with the motor-circuit whenever, as in stopping or descending a grade, the connection between the line and the motor is interrupted, and the latter is in condition to generate a current under the influence of the momentum of the train. The battery cells are connected in parallel or multiple series order during the time when they are in circuit with the generating or braking motor, so as to oppose an electromotive force of only a few volts to the charging current, thereby making the movement of the train or car instantly available for charging the battery, this charging process continuing until the speed of the armature has been so reduced by its own braking action upon the train or car, that it is no longer capable of generating the few volts necessary to effect the entrance of its current to the storage battery on the train or car. In this arrangement an automatic potential switch of special construction prevents the closing of the charging circuit until the E. M. F. of the current from the generating motor exceeds that of the battery which occurs almost instantly, and breaks the connection, when the E. M. F. of the charging current falls below that of the battery, the entire arrangement being so simple as to require no attention whatever on the part of the motor-man. The current thus reclaimed on descending grades and stops is, in the Ries system of electric railways, employed to assist in operating the train or car when ascending grades and starting from a position of rest. The battery, or such portion of it as may be set aside for operating the motor or motors, is of course discharged at a higher voltage, and can be utilized for electric lighting and all other train purposes to which it may be applicable. It will thus be seen that a twofold effect is obtained, since the surplus energy of the train, which under existing conditions is ordinarily wasted, is not only made to stop the train without the use of friction brakes, but the energy consumed in stopping the train is, with the exception of a very slight loss in conversion, etc., almost entirely recovered and turned to useful account. Mr. Sprague has stated, in referring to the operation of the New York elevated railway system, that 59 per cent. of the power expended by the locomotives on a round trip is used in starting, 24 per cent. in lifting, and only 17 per cent. in traction. That means that 83 per cent. of the total power expended upon the elevated railway system in this city is consumed on account of stoppages and grades, of which nothing is recovered, but on the other hand, the waste is augmented by the use of further steam to apply the brakes. If only one half, or say 40 per cent. of this wasted energy, can by the method above described be recovered, of which I have not the slightest doubt, the value of electric traction for roads of this character, over that at present used will be apparent, from this standpoint alone.

Another matter of considerable importance is whether one or two motors are most advisable for street car work. I myself am in favor of employing but a single motor directly geared to both axles, and it is the universal opinion of electricians and engineers that in a case of this kind a single large unit is better than two or more smaller ones, and this is especially true in regard to street car motors. A great deal has been said about the difficulty of making satisfactory connection with both axles from a single driving source, but this can be overcome, and I may say I have devised a method of doing this which avoids most of the difficulties heretofore encountered. It is a question to my mind whether Mr. Sprague's assertion that independently driven axles necessarily give better track adhesion, can be sustained. In fact it seems to me that the reverse is generally the case. There can be no doubt, as Mr. Sprague states, that independently driven axles will slip until they get the best grip, but it is also known that a sliding wheel or pair of wheels will readily continue slipping under the influence of the acquired impetus and the rotative strain exerted upon them by the motor. However, in ordinary street cars the load is constantly shifting from the forward to the rear axle, and vice versa, due to the lurching of the car and unequal distribution of the weight or load. Therefore, supposing a car to be slightly overcrowded at its rear end, the forward wheels must necessarily slip, and the motor driving them will run light for the time being, leaving the rear motor, which was designed to do but half the total work, to do it all, and subjecting it to an enormous electrical and mechanical strain. I have given this subject considerable attention, and have come to the conclusion that for street cars of ordinary construction the axles should *not* be independently driven, but should be directly connected with each other, preferably through the armature shaft of the motor itself, whose weight is equally borne by both axles. In this manner the combined tractive effect of all the wheels may be obtained in unison, which will reduce the tendency to slip, and the load upon, and work done by the motor will be a constant quantity irrespective of the shifting of the load. For pivoted truck or eight wheeled cars the objections to independent driving do not so seriously apply, but the advantages of a direct connection with both axles of a truck are even more apparent for other reasons.

I thoroughly agree with Mr. Sprague on the matter of making each car of an elevated train its own motor, provided the motors of the entire train are controlled from a single point. In all cases where stops are frequent, this plan possesses numerous advantages, but for trunk line railways having substantial road beds a single large motor, or perhaps a front and rear motor suitably connected but capable of independent operation when separated, will probably always be found preferable to a number of small ones.

One thing that struck me as very peculiar in connection with the Richmond road, was that an electrical engineer of Mr. Sprague's ability and experience should so utterly abandon all measure of precaution for the safety of his passengers and the public at large as would seem to be indicated by his employment of the track rails and ground as one side of the circuit, instead of using a double overhead conductor. This is the more to be wondered at, when it is shown by Mr. Sprague's own description of the elaborate and extensive means employed for connecting the track rails and re-enforcing them by separate wires to insure perfect connection with one another, and with the ground pipes, etc., that aside from its danger, this plan is considerably more expensive than a second overhead wire and double trolleys would be. I can only explain this action on the part of Mr. Sprague, by assuming that he has found the track return necessary to enable him to ascend the steep grades of the Richmond road, by reason of the increased tractive adhesion obtainable, due to the passage of the propelling current from the wheels into the rails. If my memory serves me rightly, it was not very long ago,—in his address before the American Street Railway Association, at their Philadelphia convention last October—that Mr. Sprague stated that on roads having grades of more than six per cent. he proposed to attach a hill horse to the motor car. Whether he intended this as a parting tribute to the usefulness of the patient and long suffering car horse who has so faithfully served the public, I do not know; but at all events he stated that he did not believe it possible for a self propelled car to get up the hill in any other way. This was some time after I had announced the results of some experiments I had made, both with the electric current and electro-magnetism, in which I was able to show and demonstrate that the tractive adhesion of driving wheels could be very largely increased by these means, sufficient in fact, to enable a motor car to ascend a grade much steeper than any yet surmounted.

However, I might state here for the benefit of Mr. Sprague and others, that this increased tractive adhesion can be obtained without subjecting the public at large to the danger incident to the use of a ground return. Every electrician knows, and perhaps no one better than Mr. Sprague himself, that a ground on any circuit multiplies the danger of leakage, and on an electric railway circuit carrying a pressure of 400 or 500 volts it should never be permitted. In the Ries electric railway system we have a cheap and simple method of increasing the tractive adhesion without employing the track rails as a return, which can be applied to any electric railway operating on the parallel system. This consists simply in passing the current from the conduit or overhead conductors, after it leaves the motor, to the forward car axle, which of course is insulated from the car body, and to connect the rear axle, also preferably insulated, to the negative or return conductor. In this way the current, after passing through the motor, must traverse that portion of the track rails lying between the two pairs of wheels, which latter are thus included in the circuit. The portion of the track rails traversed by the current will not act as a ground but simply as a good conductor inserted in the branch or loop that feeds the motor. This is only one of a number of ways in which an increased tractive adhesion may be obtained, but I prefer in most cases to employ a current that is entirely independent of the motive current and of such quality as to produce a very much greater and at the same time a variable tractive effect. I may say that we are at present engaged in applying this system to an ordinary steam locomotive, on a prominent railroad, and if the results obtained prove to be anywhere near what we have reason to expect from certain experiments made on a smaller scale, I shall hope to be able to show you a very decided increase in its pulling capacity, and to effect a considerable saving in the cost of operating the road.

With reference to the ideal method of braking referred to in the paper under discussion, I might state that a modification of our electro-magnetic traction apparatus has been applied by us for this purpose. In this we magnetize the car wheels as in our traction apparatus, but in addition provide an electro-magnetic braking arrangement, the whole being so arranged that when the brake circuit is closed, not only are the brakes applied to the tread of the wheels, but the co-efficient of friction between the wheels and the track rails is greatly and simultaneously increased, thereby enabling us to apply a greater brake-shoe pressure than now possible, and yet prevent the wheels from skidding upon the rails. This method of braking is effective in almost any weather and is applicable to street cars or to railroad trains. In cases where braking is effected by utilizing the motor or motors as braking generators, as before explained, this system is still available and only the traction circuit needs to be established between the two axles of each truck to render it effective. This in my opinion constitutes the ideal method of braking, and before very long I hope to be able to show you such a system in practical operation.

I have already occupied more time than was my intention, and though there are several other matters that I should like to have called attention to, I believe I have said all that is necessary, and will leave something for others to say while I listen. I appreciate and thank you all for the compliment bestowed in calling upon me, and for your kind and respectful attention.

MR. J. M. PENDLETON:—I rise to make an inquiry. I have heard several eulogies on the Sprague motor that is used in Richmond, and I would like to inquire whether the motor in use upon the cars in Richmond is anything but a plain, series-wound Siemens armature. If such is the case, it seems to be a very old form of motor—one that certainly possesses nothing to give it a title except the maker's name. I think we ought to give to those who have invented certain forms, the credit of inventing those forms. A short time ago I looked over a work lately published by Mr. Silvanus P. Thompson—a very excellent text-book, and one that must form a very valuable addition to any electrical library. In that work, replete with everything that an electrician would value, I found on one of the pages a very strong endorsement of the Immisch motor—a motor that is well known in England, and one which it has been attempted to introduce in this country. To my surprise I found my own name and that of another inventor mentioned on that page as having invented a motor of a form discarded by Mr. Immisch. Now, it struck me at the time that it was a misfortune that so valuable a book should contain anything to mar its high merit. A little motor intended for a very small use—a sixteenth of a horse power—was not a fit subject for such a remark; and it naturally occured to me to ask what there is in Mr. Immisch's motor peculiar to Mr. Immisch. It is certainly nothing but a Siemens armature in a two-pole field, and there was nothing indicative of the inventor about it, and the name of the motor certainly belonged only to the manufacturer. Now, in these motors, we know that Mr. Sprague has spent a great deal of time and given a good deal of study to certain lines of regulation in motors, but they are certainly not adapted to car purposes, and if a plain series motor is put upon a car why should we call it one thing or another? Therefore I think the inquiry is pertinent. Is there anything in the motor except that it is a series-wound motor such as we have been familiar with a great many years, even before we knew the names of a great many manufacturers who certainly cannot claim the invention of a motor of that class.

MR. C. O. MAILLOUX:—It was at first my intention not to say anything in the discussion this evening, but I think it is my duty to speak disinterestedly of the matter brought forward by Mr. Pendleton. While it is true that there are very few types of motors existing, and that in fact the question of selecting a type, in the mind of an electrical engineer who is about to design a motor comes down to but two forms strictly speaking, yet it does not appear to me to follow necessarily, that we should be compelled to give to the motor designed merely the name of that peculiar form. We have only to look into other fields of engineering—and I know no better example than that of the steam engine—to see how the names go. If we were to give absolutely to Cæsar what is his, the engines that we use to-day would be known by the names of people who lived before we were born. As a matter of fact, while James Watt may have done a great deal for the steam engine, yet it does not follow that he is entitled to the full credit of the steam engine, because if we were to place the engine designed by James Watt in contrast with the engine designed later by Corliss, we would find quite a difference, and if you would come down still further and compare it with the triple expansion compound engines now being made you would find a still more marked difference. Yet, strictly speaking, they are all of the same type—a reciprocating piston taking steam at both ends, and the variations merely come in the form of valve motion and various other details. Now, the criticism made by Mr. Pendleton would imply that in designing a motor you have only to look at a book and find a picture that suits you the best, and then if you decide that it is to be a series motor or shunt motor, you simply go to work and have it made. Now, to us who have been through the mill—who "have been there," as the saying is, we know that the process is far from being as easy as that. We know that there are very intricate calculations to be gone through, and calculations that must be gone through under very adverse and trying situations, inasmuch as, so far from the art being entirely known, we are in fact only on the dawn of knowledge. In other words, the data that we have to work from are very incomplete, and where facts, figures and principles end we must go often into the domain of the hypothetical to help ourselves out. Now, I think that so far from disparaging the efforts of people in designing, we should on the other hand, certainly give them credit for what they have done, because we ought to realize the amount of study, hard work, experimenting, and oftentimes expense which has been necessary to enable them to arrive at those results. I speak feelingly on that subject, gentlemen, because as I have just said, I have been there myself lots of times, and I know what it is to design a dynamo-electrical machine or an electric motor, and I also know how it is to strive to accomplish results under difficult circumstances. So I think that just as we give credit to designers of steam engines and designers of other things, who simply re-arrange details and use their best skill as engineers, for the work which they have done, so we should give credit to electrical engineers for the work that they are doing; and I claim that if Mr. Sprague or anybody else is able as an electrical engineer to combine material and to mould it so that it shall accomplish results that nobody else has accomplished, that he is perfectly and fully entitled to have the motor known by his own name, just as much as a steam engine is known by the maker's name. (Applause.)

MR. PENDLETON:—I think, Mr. Chairman, in the matter of the pleadings of my friend Mr. Mailloux, that he is always very ingenious, and very kind in matters pertaining to inventions, although he did not directly reply to my question. What I meant was that where a man has really become an inventor and a student in certain styles of regulation and has combined with this, other patents and other matters, he is entitled fully to the benefit of his work and to have it known as his work and his style of apparatus; but where he departs from that to another type, why should that name be continued when it is nothing of his work, and belongs to somebody else. That is what I spoke of. I did not intend to throw a bombshell into the controversy.

It was a mere inquiry. I heard the matter lauded and although I know the work of Mr. Sprague, his very intelligent and you may say audacious attempt to overcome the natural difficulties of a city having the grades of Richmond, for which he is entitled to the highest credit; yet at the same time I thought that it might be well to distinguish what belonged to one and what to another. There is no regulation in the motor of the peculiar type that we know of belonging to Mr. Sprague and which is particularly his property. I saw a short time ago in the *Electrical Review* of London, a remark that in America the very slightest difference seemed to create a system. It seemed to me a very appropriate remark and hence my simple inquiry whether that was a simple series-wound motor of the ordinary type that was used.

THE CHAIRMAN:—I do not think there is any danger of Mr. Sprague's reputation for his distinguished service in the matter of street railways suffering any harm in a meeting of the Institute of Electrical Engineers; but I think we might better return to the discussion of the subject of the evening—Mr. Sprague's paper, and the criticisms that have been presented upon it here to-night.

I hope other gentlemen will take part now in discussing the papers. Some interesting points were noted by Mr. Leonard particularly in regard to what he considered the deficiencies of the overhead system and the relative merits of the storage battery, and there are gentlemen here, I think, quite well acquainted with those topics or any other points brought forward in the original paper or the papers read this evening.

DR. WHEELER:—I see that Mr. Sprague is present. Perhaps he can add something to what he said last spring on the subject, and perhaps reply to some of Mr. Leonard's remarks.

THE CHAIRMAN.—My defective eyesight will have to be my excuse for not seeing Mr. Sprague. I hope he will speak to us at this time.

MR. FRANK J. SPRAGUE:—I am rather glad that some argument has come up in the discussion of the paper. I think possibly I am in a position to correct some of the impressions which have been received so as to remove doubts about certain points. I do not care to enter here into a discussion as to whether any work that I do shall go under my name or in some other man's. So far as the difference between the Sprague motor in Richmond and the Siemens machine is concerned, either Mr. Pendleton does not know what the Sprague motor in Richmond is, or what the Siemens motor is. We use in Richmond a double commutator machine which is unknown to-day to any one except those whom I have taught. Its plan of winding has never been published or described. It is, as I say, unknown except by the employees of my company, and if there is any question as to the similarity of that winding to some other, I have not the slightest objection to giving the necessary diagram of connections. It is a series machine, but a series machine which has some peculiar characteristics. It is wound with three sets of coils, and the pecularity very largely consists in the method of connecting, and certain switches, by which the coils in these machines are commutated and reversed, their field magnetization and their current varied at will, through progressive changes, by means of which, when desirable, we are enabled under various loads to maintain an almost absolutely constant field, while the current may vary 300 per cent. That has never been done with a motor before; that is, to so regulate the field that, with a variation of 300 per cent., a perfectly constant field will be maintained. This field connection is somewhat peculiar, so much so that it would be useless for me to attempt to describe it now, although I shall take occasion to publish a description of it in the near future. There are, however, some characteristics of motors other than the mere winding of the armatures or fields, or their regulation, which are essential for street car purposes. It is, as Mr. Mailloux says, a question partly of proportioning, partly of calculation, a putting together of those things which you have at hand to meet those conditions under which you have got to work. Two years ago, I think I may fairly say, such a problem as equipping the Richmond road, with its abnormal conditions, with the circumstances of manufacture which we had to meet, and in the time that was given to us in the then undeveloped state of this art, would have been looked upon as somewhat preposterous. I am frank to confess, that, had I known what I had to go through, I would, perhaps, have had neither the necessary courage, time or patience, or have cared to have risked the money which I did. I have spent in Richmond $150,000 and I have learned something. While I do not care to state all that I learned, I have taken advantage of it in our future work. A great many of the principles of regulation that have been associated with my name are not characteristic of the Richmond machines. They are designed for other purposes. Now, referring to Mr. Leonard's paper, a great many of the deficiencies he speaks of exist in Richmond, and unless the machines were of superhuman design, they must exist there. A year ago those machines were put in in a great hurry. The maximum normal capacity that they ought to run at is about seven and a half horse-power. Very frequently they have been called upon to develop ten or eleven horse-power, an increase of about 50 per cent. In the nine months during which this road has been in actual operation, and it has run nearly half a million of miles and carried millions of passengers, not one working car has ever been inside of a closed shed. They have been housed in the street. The photograph which you have in the report of the paper read by me some months ago gives you some idea of where the cars have been kept. Of course, you are aware that when a machine is running it is exposed to more or less wear. It needs something of care and attention. A locomotive that makes two or three hundred miles, is sent into a round-house, thoroughly cleaned and taken care of. It is a very good locomotive that will run with safety, and it is very poor management that will permit it to run more than three or four hundred miles without looking it over. The Richmond machines have run sometimes as high as 6,000 or 8,000 miles without being sent inside a covered shed. They have run without covers of any kind or character. They have run with six inches of water in the street and brake-beams running in the water, which was splashing on the motors. They have run until you could not tell what the machine was; until the accumulation of mud and oil and filth was of such a character that no other piece of machinery in the world would ever have run. I am not responsible, unfortunately, for the management of the Richmond railway. If I were, we would not have the possibility of a number of cars lying in the car shed, so-called, unfit for duty, after having been left in the streets during ten days of rain. The report from one of my own inspectors less than one week ago was that there was no more trouble in running a 40-car road by electricity than one operating three or four cars. He is a level-headed, practical, cautious man, not given to strong statements and very conservative in what he says or does. He says that out of 30 cars equipped for duty, there is no difficulty whatever in maintaining 29 on the road, if only that legitimate, ordinary, reasonable care is given to the machines which ought to be given to them; and that is the opinion of others who know the exact circumstances of the Richmond management, and the character of the attendance which the machines have had. There is no electric car in the world which has ever made anything like these records—from 80 to 110 miles a day on grades as high as 10 per cent., under loads 50 per cent. above the normal capacity of the machines, in the hands of the men oftentimes appointed for political purposes and not for intelligence, on duty twenty hours a day, and making 6,000 or 8,000 miles without going inside a shed to be cleaned off. I have been called a crank, and some time ago when I made the statement, as I did in the paper that was read here, that we would get to that position in electric motors in which it would be almost an impossibility in ordinary use to break them down, and that I proposed to wash machines down occasionally, to get them clean, the remark was made, "Well, that fellow must be a damned fool." Well, I quietly pocketed that remark, and within the next thirty days or so we will have the machine out, and we will put one of those armatures that people have laughed about inside a barrel of water for twenty-four hours, and we will take it out and we will run it. There is no piece of electric machinery in existence to-day that will stand it. One of the difficulties which will occur on any road where cars are not properly and decently housed some part of the twenty-four hours is this: Cars that are running, of course, warm up, the pores open, they are ready for the absorption of any moisture. If it comes on a cold, damp, murky night, and these cars are left utterly unprotected, they will absorb more or less moisture. In dealing with the potential we are using there of 400 to 450 volts on the line, you have a possibility of a short circuit in the coils. Some of the difficulties Mr. Leonard mentions I think he might have recognized as of a purely mechanical nature, or difficulties due very largely to lack of ordinary care. The breaking of a trolley wire, sometimes caused by a trolley catching in it, can be, of course, almost entirely obviated by one or two precautions; one of these is dispensing with solder in the erection of a line. In putting up the Richmond line we used hard drawn copper. Hard drawn copper as it comes out of the mill has a remarkable tenacity, but when it has been handled, and when under the heat of a soldering iron it has been warmed up, you reduce it practically to the strength of soft drawn copper; so that you will have at the point where solder has been applied a weak spot which entirely nullifies all your other precautions. In our future work, this trouble will be entirely removed, because we will not use solder from one end of the line to the other. The use of silicon-bronze or aluminum-bronze drawn for high conductivity and great tenacity, will likewise render less possible any accident due to the breaking of the wire. This wire can be drawn of from 100,000 to 150,000 pounds tensile strength to the square inch; and being put up without solder, I can see little possibility of that wire ever coming down.

So far as the use of poles is concerned, if you use a green pole, as the poles in Richmond were, it will bend; if improperly set in a clay soil it will get out of position. If it is properly seasoned and set in concrete there is no reason why it should give trouble. One of the most important roads that we have—we have some 26 or 27 now—is the West End road of Boston. Rights for an overhead system there having been granted within a few days, we are putting up a line that costs four times as much as the Richmond line did per mile. I believe I said in my recent paper that the objection to overhead wires was entirely reasonable, because people did not put money enough into them. Having put all the money which I did into the Richmond road, I do not propose to put in any more there. On a new road, where I can make my own conditions before the contract is signed, I am willing to put in just as expensive a road as is desired. Instead of putting in wooden poles in Boston, we are putting in iron poles tested up to 1,400 pounds. Instead of setting them in clay, we set them in concrete. Those poles will not come down. So far as the span wires are concerned there is no danger of their breaking.

As to the use of a single overhead line that I know is a much discussed question. But I find the practice of most people is a single overhead line. There has been a good deal of kicking on the part of telephone companies. It has chiefly disappeared down in Richmond. The trouble is that the telephone companies had the earth and they wanted to keep it.. (Laughter). I can see no particular reason why a telephone company should not be required to exercise the same degree of care in putting up their lines that they would if the electric railroad were already in existence The telephone man runs out his circuit as far as he can and charges as much as he can, and then, because some poor railroad company wants to put in some cheap and effective system of operating its cars, I do not think that it is quite just on the part of the telephone companies that they should raise their hands in such holy horror over the use of the earth circuits. Mr. McCluer, of Richmond, who was mentioned, had ingenuity enough to get out of his troubles. He took the trouble to run over his main telephone circuits, where they came near our wires, a single return wire, and he makes his telephone connections to that. His troubles have disappeared. I suppose it might have cost his company a few hundred dollars. If he objected to paying that, I suppose we could have paid it for him; but he has, as I say, overcome his troubles by the most trivial possible expense. If that is the only objection that telephone companies have got, it is not a legitimate objection. So far as the burning out of their exchange is concerned, I will have to correct Mr. Leonard in that. At the time one of their decayed poles fell down and landed three or four hundred wires on top of our lines, the dynamos were not running at all. This is the only pole breakage I know of. We have burned out some of their instruments, but that is simply because we took a reasonable amount of precaution, and they did not. They got their wires across ours, and they got the worst of it. The great trouble with telegraph and telephone business is this—the thousands of miles of dead wires hanging down in the streets and over the house tops. A man who has a telephone does not take the trouble to take down his telephone wire after he has discontinued the service. There is no æsthetic sentiment in a telephone company. They are perfectly willing to say: "Here, you electric light people and you electric railway people are causing a great deal of trouble to our customers." If they would all join hands in an equitable arrangement, as some of them have, they would have little cause to complain.

A criticism has been made about the use of two machines on a car. I have spent half an hour trying to get the length of this room with one motor on a 6 per cent. grade, under a condition which happened to give a slimy surface on the rail, and I have seen under the same conditions, on a 10 per cent. grade, with a worse track, two motors handle the heaviest loads. It is not merely because the wheels can get a better grip—you have got your total weight available for traction. The forward wheels can also in case of an emergency travel faster than the rear wheels. That has been done—even on a level, and has the advantage of breaking up whatever obstructions there are on the track and leaving the track perfectly clear: if you put between your wheels a broom or brush you have excellent traction It could not be done by gearing both wheels to the same motor, simply because you could not get that independence of grip that is necessary. Running both pairs of axles requires certain devices, the use of which, in view of existing street car practice in the matter of constructing running gear, I do not think advisable. There are some 40,000 or 50,000 cars in the United States, and there are one or two settled principles in them; one is, perfectly independent action of the axles. No four wheels on a car are of the same diameter; they are never cast absolutely alike; they do not run necessarily on the same tread. Any difference in the level of the streets will send the weight of a car down on one side, in fact, it seems to me, in the face of all I have seen, almost trivial to make any objection to the independent driving of two axles. I admit it is cheaper to build one machine to put on a car, but you can build two smaller machines of the same capacity. Space in a street car is a matter of considerable importance. You want to get large power on a car to handle heavy loads, but you have got to have it in comparatively small space.

As to keeping a trolley on a wire, I am quite willing to enter into a contract to put a trolley on a car which never can come off the wire, and, in fact, some of our cars are now running with one man, and have to run with one man, simply because the railroad people won't pay an additional conductor.

The methods of getting rid of troubles have been, of course, subjects of a great deal of study. It was not possible a couple of years ago to look forward and face all the conditions that arise in practice. We electricians did not know enough about street car practice, and street car men did not know enough about electric railways, so we had to join hands with the street car men, and we had to put the cars and motors into the hands of ignorant men and meet those difficulties in order to know how to get over them. I think I may safely say that the road we put in. sixty days from now will bear about as little resemblance in details to the Richmond road as it is possible for any two roads to bear to each other, simply because of great improvements, not only in the motors, but in the gearing, switches, trolley work and all overhead superstructure.

A two-wire system requires from two to four times the weight overhead. Where we put a main conductor alongside of the street and reduce the trolley wire to a single one a fifth of an inch in diameter, that enables us to keep our structure in position a great deal better. So far as safety is concerned the fact that a horse was killed in Richmond in the early days of running is no criterion of it. Two wires are more liable to come down than one; but you won't get killed with 400 or 500 volts. I have several times taken that potential myself. We don't know whether the horse that was killed in Richmond broke his neck or was killed by electricity. He was boiled in order to get his skeleton; he did have a broken neck; it is not known whether that killed him. We had a mule there. (Laughter.) A man threw a lump of ice down on to our wires. The first mule—two of them were being driven by a darkey, ran his head into the wire and kicked and finally fell over and lay on the wire. About half an hour after that they telephoned up to the general manager's office, and said, "We have got a mule pretty well used up by a current; we want damages." He said, "All right." About 48 hours afterward they sent up word that they did not think they wanted any damages; that mule seemed to be a great deal livelier than before. (Laughter.) In the case of the horse, that was gross carelessness on the part of everybody concerned—the conductor who broke the wire, the dog who investigated it, and the boy who rode the horse, but the only one injured was the horse.

The statement that the Richmond Company has never been able to run more than thirty cars out of forty is an error. The Richmond road never expected to run more than thirty cars out of forty. Its plant was built for thirty cars. A forty car motor equipment had nothing to do with their line work, or their central station work. It is perfectly feasible as a matter of fact, to run the entire forty cars.

As far as the use of the storage battery is concerned, I am just as anxious as anybody else that the storage battery should be a success. But it will never get on a car in Richmond when they have got to put 4,000 pounds weight on that car. They never will take 20,000 pounds weight up a ten per cent. grade with a storage battery until it is in a good deal higher state of efficiency than it is to-day. We have, although not prominently connected with storage battery work, been experimenting for a long time. We keep a car on tap. We have had built hundreds of plates in the past year, trying to devise some means of overcoming some difficulties that exist in storage batteries now. We hope to have a normal capacity of discharge in, say a 35-pound cell of not less than 75 or 80 ampères, and ability to stand 150. The resistance ought to be about one third or one-fourth of the present storage battery. The plates will be practically indestructible; and, finally, it won't be a Faure cell. That is the sort of thing I am looking for. I do not know whether we will get it. The results, so far, have been very encouraging, but we never will attempt to climb a ten per cent. grade with it, and we never will put it on the market until we know it is satisfactory. We will do our own experimental work. We expect to get the battery down so that not more than 1,800 pounds is necessary to handle the car under ordinary conditions. That will be something of an advance. The credit will not be mine, except that I shall pay the bills. Two years ago it was not possible to design a street car motor for entirely perfect work, I do not think enough was known about street car work. Certainly we are in a position today to design a great deal better machines than we did then. I shall have the pleasure of showing and explaining to this association in the near future our two latest machines—one about 7 1-2 horse-power, the other about 15—and their construction, I think, will appeal to street car people at least. I think by the time the Boston road is running, which will be in about five or six weeks, if Providence is not against us in the matter of the weather, we shall be able to show such improvement to the overhead line, as to its efficiency, economy and appearance, the character of the motors and the work which they will do, as will be a subject for mutual congratulation.

MR. RIES:—I should say in regard to the advantage of using one or two motors for propelling cars, that in ordinary street cars there is apt to be a lurching motion and weight is shifted from the front axle to the rear and *vice versa*, and when each axle is driven independently, if the rear platform, for instance, is more heavily weighted the front axle revolves lightly.

Therefore, the work is on a motor that is only half large enough to do it. So far as the slipping of wheels on the rails is concerned, necessitating cleaning the track in winter—that can be accomplished by placing brushes in front of the forward pair of wheels. The principal objection to the use of two motors consists in the fact that it complicates the machinery. On a road, operating say, 40 cars, it requires 80 motors where 40 would do the work. It is not at all necessary in the use of a single motor to interfere in any way with the present existing construction of the running gear of the cars. It is just as easy to apply a motor to the present cars without making any change whatever and gear that motor direct to the two axles as to give each axle an independent motor. In fact I have developed such a system which avoids the difficulty of removing or changing any portion of the car.

MR. L. F. LYNE:—I would like to ask Mr. Sprague if he really meant that a locomotive only runs 150 miles before it needs repairs. I think he meant 1,000. There are reporters here, of course, and if they get that wrong it will not sound well. I unstood Mr. Sprague to say 150 to 200 miles.

MR. SPRAGUE:—I might as well correct that impression at once, if you will permit me. I said it was not practicable in locomotive work to run over 200 to 400 miles without the locomotive going under shelter and being properly oiled and cleaned; I did not mean going into the repair shop. What I say about the Richmond machines is this: They are not put inside of four walls; they are simply left in the streets. You cannot leave a locomotive or any other machine in the street for eight months under all conditions of the weather and have a machine that will not be apt to sometimes give you trouble.

I was quoted a little while ago as making a remark about hill horses. I was asked by a man who was interested in storage batteries—I believe he lived in New Jersey—if I would recommend in the equipment of a road which ordinarily did not have more than two or three per cent. grade, but which did have in some particular places a heavy grade of eight or nine per cent., the use of motors which would climb a nine per cent. grade. I said that as a matter of commercial investment I would say no. I said: "You will find it cheaper if, when you get to that eight or nine per cent. grade, you take the trouble to put on a horse." That, certainly, was the intention of my remark.

So far as Richmond is concerned, I did not expect in the first place, that we were going to have a ten per cent. grade there. Our specifications were for eight per cent. We have in some places the equivalent of a thirteen per cent. grade. We develop a remarkable traction, a traction which I never anticipated would be gotten out of a car; but no matter how we magnetized our wheels there, unless we drove both axles we never would get up these grades at all times.

MR. LYNE:—There are one or two points in the discussion which I would like to refer to. The devices, so far as I have been able to discover, for transmitting the power from the motor to the shaft of the car or axle of the car, have been, in many cases obsolete devices. For instance, the grooved friction wheel has been used on the "Ben Franklin" that was tried on the 9th Avenue road. That device was abandoned by engineers years ago, for the reason that if a sufficient pressure was not brought to bear on these wheels to bring the friction up to the maximum point at once, the surfaces would slip on one another, and produce a flat place. When the Fontaine locomotive came out we were led to believe that the whole railroad system of the country would be revolutionized in a few months by it. A certain few predicted its utter failure, and it failed, principally in consequence of the tires getting flat places on them. As soon as they did get a flat place they began pounding themselves to pieces, and the result was a dismantling of the whole affair. We know that the same occurs in locomotives in climbing grades. They slip, get flat places, and then the tires have to be turned off.

I am glad Mr. Sprague is here to-night and that he has so fully explained the difficulties which have been shown to exist in an overhead line. I fully agree with him that an overhead line must be put up as if it was never coming down, or else it had better not be put up. It is an old principle in railroading that each train should have its own independent motor, whereas, if you have a cable or a wire upon which a number of trains depend for propelling power, when that line fails or any detail fails, you stop your whole road. Now, we have either got to have the overhead line a permanent structure like a bridge or a railroad structure, or else you have got to resort to some means like the storage battery to give each train its own independent power. Then, if it fails, you can throw it one side and let the other trains move on.

I want to say just a word in reference to the storage battery. While I do not believe fully in the storage battery as it is constructed at the present time, I do know that it has been misrepresented in a great many instances. I have been told that it does not give as great an efficiency as is represented. Now, we have just tested some storage batteries, and we have got 80 to 85 per cent. of the charge back, and the failure of some of the batteries—we put in two series in which the plates failed at the end of about three and a half months—was due to fault in the material. We put two other series right in the same place with improvements that I had suggested from the experience we had with the first batteries, which consisted in placing the plates five-sixteenths instead of one eighth of an inch apart. Then the scales, when they formed, went directly to the bottom instead of lodging across the spaces between the plates, and short-circuiting them. It was this battery that we tested the other day and got 86 per cent. We have had it in use four months with no trouble or short-circuiting whatever. But with the others, with plates about one-eighth of an inch apart, the scales were formed the full size of the little square, and they would tip over and lodge across the two plates. We have to keep at work at them all the time picking out the scales. There is one other improvement which has been suggested and which I think will be adopted. The scales fall to the bottom, and lodge across the wooden bridge or support upon which the plate rests. That can be overcome by simply making one series of plates with a leg at each end standing on the bottom of the cell and the other series of plates resting on a hard rubber support placed on top of the plates that rest on the bottom of the cell. That leaves the space entirely clear for the scales to fall to the bottom. I have great faith in the storage battery, and I believe that a storage battery will be made eventually which will give a good efficiency and be much lighter than the ones now made.

MR. SPRAGUE:—The question of depreciation of line is a question of construction very largely. It is certainly not a question of wear. A consideration of the actual time in which a wire is in contact will show this. With cars that are making two minute headway and running seven miles an hour, with a round trolley wheel, making contact on a round wire, we have almost a line contact, but call it an eighth of an inch contact. Any portion of the wire is in actual contact with the wheel less than one-two-hundred-thousandth part of the time; in other words, in continuous current contact, not over one hour in twenty-six years Consequently the current ought not rapidly to wear the wire out, and it does not. In fact the period in which any particular part of the wire is in contact is not over one-two-thousandth part of a second when the car is moving seven miles per hour.

The question of insulation, leakage and lightning came up a little while ago. It is a remarkable fact that lightning does not necessarily always follow the single overhead wire. We have had in Richmond quite a number of cases of the line being struck by lightning. Our ground circuit is made by three paths; two through the boilers and water connections, and a third through a covered wire running about 1,000 feet on poles out, and direct down, to the track. We have had lightning come over that wire several times, and the statement of the engineers is that it is about an even thing whether it comes in on that line or the other one. It terminates in a well, and as a matter of scientific interest the engineers sometimes watch it as it goes into this well. So far as the leakage is concerned on that line it can easily be reduced to an average of one-tenth or one one-hundredth part of one per cent. It has never been sufficient to cause the slightest interference with the operation of the wire except in the case of the short circuit. If existing, it can very easily be perceived by taking the reading of the ampère meter. The leakage is nothing so far as loss of power is concerned.

MR. DAVID E. LAIN:—If anything that I may say from what little experience I have had with electric locomotives will add anything to the success of this meeting, I shall be very glad to give it.

About the only point that I care to consider at the present time is the means of coupling the armature of the motor to the driving wheels. In Mr. Sprague's excellent paper he classes the method of direct coupling with those others, coupling by means of chain gear, friction gear, etc., in a way which, I think, is rather hard on the direct coupling. I have had some experience with some of these methods, and it seems to me, as far as I have observed that the plan of direct coupling offers certain advantages which no other method has. The easy motion and perfect noiselessness of the working of this coupling certainly places it ahead of any other means, and its mechanical simplicity is greater than any other, all the details being reduced to a minimum. The objection usually urged is that on account of the slow speed at which the armature must necessarily revolve, running no faster than the drivers, a larger machine is necessary than where the armature may revolve several times as fast as the drivers. This may be an objection and may not. In street railway work, where certainly not more than ten horse power is needed for each car, where the whole equipment should not weigh more than say 5,000 pounds when the car is loaded, the weight of the motor enters as such a small part of the whole weight even with a direct coupled motor, that it is not of such great importance after all. If the drivers be reduced in diameter to say 18 inches or thereabouts, a motor can be built that is not necessarily more than 25 per cent. greater in weight than one using gear of a ratio of 1 to 12. The smallness of this difference is due, in part, to the greater efficiency of direct coupling as compared to gear. Of course transmission by means of gear is not possible where speed is reduced or changed from say one to twelve, or one to ten, without some considerable loss of power. All this is saved in the direct connection where the loss by transmission should not be more than a very few per cent. Yet we must pardon Mr. Sprague for claiming that his particular system is better than any other, for we are all willing to concede this is but another proof that he possesses the necessary qualities which go to make up a successful inventor, and that Mr. Sprague has also a greater knowledge concerning his system than any other. Yet I think before the close of this year, at least before the close of next year, results will be obtained with direct coupling that will show that a direct coupled motor is quite efficient even for small work like street car service; and if the mechanical efficiency and reliability of this method be also considered, I believe it willl be found to far surpass any other means yet employed.

Mr. Leonard:—With reference to the statement in my paper regarding the burning out of the exchange, that report was based on information I received from the telephone manager's office yesterday afternoon.

In regard to the difficulty and trouble experienced by the telephone exchange people, on account of the ground circuit of the electric railway, it has been largely, if not altogether remedied, by the erection of a metallic circuit, or what amounts to the same thing, a large copper wire to which the telephone wires are grounded instead of going to a gas pipe as they ordinarily do. The difficulty has been obviated in that way, but there are quite a number of private telephone lines put up by various people, including our railway company who cannot afford to go to the expense of putting up metallic circuits and the result has been that we have been obliged to discard our private telephone line and pay tribute to the telephone company and their metallic circuit system.

With reference to the mechanical difficulties which Mr. Sprague alludes to and which of course have been improved upon elsewhere since the road was first erected, it is true that those mechanical difficulties can at once be perceived by personal observation, and should have been remidied as far as possible at the outset, but the electric road at Richmond was put forward as being a sample of the highest development of the problem thus far attained, and of course we could only take what had actually been done and criticise it accordingly. This I endeavored to do in the few remarks I made. That the work is susceptible of improvement there is at once apparent to almost every one, and that Mr. Sprague has in other cities succeeded in improving on the overhead system is nothing more than what would be expected from the ingenuity and skill with which he has overcome all the difficulties that were encountered in the Richmond enterprise.

THE CHAIRMAN:—We should be pleased to hear from Mr. Rae on this subject.

MR. THOMAS WHITESIDE RAE:—Mr. Chairman, there is nothing that I have to say in addition to the remarks that have been made to-night, but I would like to record my hearty concurrence with Mr. Sprague's dictum respecting overhead lines. It is simply as with most engineering questions, a financial rather than a technical one. One may always have a good, durable, sightly overhead line by paying sufficient money for it. As at present constructed they do develop obstacles of various kinds and certainly are not as symmetrical as they might be, and do lack the perfection that might be given by the expenditure of more money. It is quite possible, I think, to make use of an underground conduit at perhaps no greater expense, (certainly no greater when one considers the low cost of maintenance of a properly built conduit) than a properly built overhead system would entail. The difficulties which Mr. Leonard refers to I think cannot fail to be recognized by everybody of experience of such matters, but that they are inherent to the system does not necessarily follow, as Mr. Sprague has pointed out very clearly.

MR. SPRAGUE:—I think we will know much more about it when we have gone through a Northern winter which we shall do very soon. We have cleaning cars and we shall put ample power behind them and can run much faster than with horses because we have the power behind us rather than ahead of us.

DR. WHEELER:—Mr. Sprague in speaking of the liability of the wire to wear on account of the current, mentioned the fact that it could not possibly wear because it was only in contact at one point a very small part of the time. But I should think that there would be considerable wear due to the constant sparking that would occur when the roller hops a little bit off the wire.

Mr. Sprague:—It is not that to which I referred. It is not the same as a sliding contact on the wire. Your wheel is turning all the while and there is no difference of potential between the part you leave and the part you are going on. You have a good contact where the trolley is held up by a good spring; you may inspect that wire by a microscope and detect very little signs of wear at all. There would be a very slight frictional wear, and it is probable there is a certain percentage of slip of these trolley wheels on the wire. I have, after eight months wear on the very heaviest portion of the line, very carefully inspected the wire, which was chemically cleaned, and I was agreeably surprised to see the small evidence of depreciation. If there is no sparking then the wear is entirely due to friction. The fact is the wear is remarkably light.

The Chairman:—We should be very glad to hear from some member of the Society which has been our host here so long and allowed us to use its rooms. I believe Mr. Emery is present and might like to say a few words to us on this subject.

Mr. Charles E. Emery:—Gentlemen this is a complete surprise. I came here to learn, not having kept myself informed of the advances made in the propulsion of street cars by electricty. I have been very much edified and congratulate the gentlemen present, not only on the grand advances which have been made in electrical science, but on the growth and progress of this society where you can interchange views on the various branches of the subject so interesting to yourselves and to us all, and I trust that the society will grow to be one of the strongest and best in the country. Thanking you for remembering the American society of Civil Engineers and with personal thanks for your kindly reception, I will ask to be excused. (Applause.)

The Chairman:—Have any other gentlemen remarks to make?

Mr. Sprague:—I would like to make one remark in regard to lightning. We had the line struck by lightning several times, and we have had motors on the cars burnt out, but I believe I may safely say now that we have got lightning pretty well harnessed. I believe now there is no difficulty whatsoever in absolutely and perfectly guarding a car or a central station, so that lightning simply cannot get to the machine. We had three darkies sitting on the back dash-board of a car; one of them was sitting on the switch. The lightning came in, and grounded through the switch. Those three darkies jumped off the cars at a very lively pace, but there was no danger. We have had the lightning come in and ground through the switch at the time the motor men were running the car. They paid not the slightest attention to it. They simply heard the click—that was the end of it. It will now be perfectly guarded against. It cannot get into our machines, and it cannot get into our stations; at least if it does it is only one time in 10,000, and then it goes to the earth harmlessly.

THE SECRETARY:—The Chairman of the Committee on Papers and Meetings has an announcement to make regarding the next meeting.

MR. T. C. MARTIN:—The next meeting will be held on November 13th. The subjects are as follows: Prof. W. E. Geyer of the Stevens Institute will read a paper on "The Geyer-Bristol Meter for Direct and Alternating Currents." We have with us this evening a gentleman, M. Abdank-Abakanowicz, whose acquaintance I think you will all be glad to make. He is one of the leading electricians and mathematicians of France; he is also well known as one of the leading contributors to the principal electrical paper in France "*La Lumiere Electrique*." I am very glad to announce that on the same evening that gentleman will favor us with two papers, both of which will be accompanied by experiment and demonstrations; one will be on the Carpentier electrical method of recording and reproducing music; the other will be on the Abdank magnetic call.

MR. MAILLOUX:—I think it might not be out of order to suggest that Mr. Abdank give us some views on the subject we are discussing now.

THE CHAIRMAN:—I am sure we should be most pleased to hear Mr. Abdank.

MR. MARTIN:—I would like to say on the gentleman's part that he speaks our language extremely well, but that he has a tremendous amount of modesty. Therefore I think he will appreciate it if we let him off this time, and tax him the more heavily at the next meeting.

Adjourned.

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OF

CURRENT ELECTRICAL LITERATURE.

It is the intention to include in this Index, month by month, as far as desirable and possible, all the articles on electrical subjects, of scientific and technical importance, published in electrical and other journals, and the papers read before the various technical societies. The titles will be classified so as to present at a glance the range of current discussion and the advance of investigation or discovery in the different fields of work.

The Secretary will be under obligations to any member who may supply, or call attention to, any omission noted, and will be glad to have suggestions tending to render the Index more valuable now and for future reference. He will be happy also to co-operate in this respect with members prosecuting special studies and experiments.

The Index sheets are printed separately so that they may be detached at once, if necessary. They will all be bound together at the end of each Volume of Transactions.

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The History of the Phonic Wheel and its Application to Synchronous Multiplex Telegraphy. (Ill.) Paul La Cour. *Lond. Elec. Rev.*, Oct. 7, (cont'd.)

Springs on Telegraph Lines. (Corr.) W. H. Preece; C. Bright, Jr. *Lond. Elecn.*, Oct. 28.

Springs on Telegraph Wires. (Ill. Corr.) F. Caws. *Lond Elecn.*, Oct. 21.

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A New Repeater Arrangement. (Chesley.) (Ill.) *Elec. World; Lond. Elecn.*, Oct. 28.

Multiplex Writing by **Electricity.** (Note.) (Garel.) *Eng.*, Oct. 21.

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On Induction between Wires and Wires, (Ill.) W. H. Preece, Brit. Asso. Ad. Sci., Sept.; *Elec. World*, Oct, 15; *Lond. Elecn.*, Oct. 7.

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On the Localization of a Complete Fracture in a Submarine Cable by Bridge Measurement to Instrument Zero. (Ill.) A. E. Kennelly. *Lond. Elecn.*, Oct. 14, Oct. 21.

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Was Philip Reis the Inventor of the Articulating Telephone? E. J. Houston. *Jour. Fkln. Inst.*, Oct.

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Unsteadiness in Arc Lamps. F. de Wolffers. *Lond. Elec. Rev.*; *N. Y.*
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son. *West. Elecn*, Oct.—

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Mech Engr's. *Rail. and Eng. Jour.*, Oct.

The Electric Lighthouse on the Isle of May. (Ill.) D. B. Steph
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A Novel Magnetic Engine. (Ill.) C. K. McGee, *Science*, Mar. 7, 1884. *Elec. World; N. Y. Elec. Rev.*, Oct. 8.

The Pyromagnetic Dynamo. T. A. Edison, Am. Asso. Ad. Sci. *Rail. and Eng. Jour.*, Oct.

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Pumps driven by Elwell-Parker motor, Trafalgar mine. A showing in favor electricity over all other methods, for distances above 1000 yards.

The Electric Motor in Building Operations. (Ill.) *Elec. World*, Jan. 7, p. 5.

Use of Sprague motor at Topeka, Kan.

The Patten Electric Motor. (Ill.) *Elec. World*, Jan. 7, p. 7.

Combination of Daft Motor and Worthington Bucket Pump. (Ill.) *Elec. World*, Jan. 14, p. 14.

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Gearing for street cars; motor pivoted on axle.

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Street Railroad Traction in New York. *N. Y. Elec. Engr.*, Jan., p 27

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The Daft Electric Railway System. (Ill.) *St. Ry. Gaz.*, Jan. p. 5.

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As to evidence that ether near the earth's surface moves along with it.

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Thermostatic arrangements for alarm and release.

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An Electric Piano. (J. Webber.) *Eng. Post; West. Elecn.*, Jan. 7, p. 9.

The Star Electro-mechanical Gong. (Ill.) (Elec. Gas Ltg. Co., Boston.) *West. Elecn.*, Jan. 7, p. 9.

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An Electric Sleep Preserver. W. Maver, Jr. *Elec. World*, Jan. 7, p. 3.

A Scientific Toy—A Magnetic Oracle. (Ill.) *La Nature; Sci. Am.*, Jan. 7, p. 5.

An Electro-Magnet Made of Two Cannons. (Ill.) *Sci. Am.*, Jan. 14, p. 15; *Elec. World*, Jan. 21, p. 27.

15-inch Rodman guns; weight 50,000 lbs. each; wound with eight miles of insulated wires. Total weight of apparatus 130,000 lbs. Current from a 20-light arc machine.

Electrical Litigation. (Ed.) *N. Y. Elec. Engr.*, Jan., p. 1; *Sci. Am.* Jan. 21, p. 40.

A comprehensive review of patent suits in progress.

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Liquid in a tube repelled by magnet. If tube be inclined slightly, the magnet acts as a brake, stopping the flow.

The Convection of Electricity by Steam. (Von Lang and Lecher.) Vienna Acad, Sci. *Sci. Am.*, Jan. 14, p. 25.

Experiments showing that an electrified liquid vaporizes more rapidly than one not electrified.

Soldering Electrical Conductors. (Prisiajniky.) *Sci. Am.*, Jan. 14, p. 25. Sal-ammoniac mixed in with the melted solder; one ounce to the pound of lead.

Electro-Metallurgy by Dry Way. (R. K. Boyte.) *Sci. Am.*, Jan. 14, p. 25.

Object to be galvanized is covered with a sheet of the metal that is to furnish the deposit, and an intermittent discharge is sent through the junction.

Dangers of Overhead Electric Wires. *Sci. Am.*, Jan. 14, p. 20.

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Some Recent Annunciators. (Ill.) Elec. Gas Ltg. Co., Boston. *Prac. Elec.*, Jan., p. 138.

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Batteries in which platinum or carbon plates are immersed in communicatin fluids capable of undergoing chemical reaction.

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Letter from R. Clausius as to his authorship of the theory; with note b Dr. O. Lodge.

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Description of various solutions of iron electrolyzed by the writer, and o their character of deposit.

Nickel Plating. *Sci. Am.*, Jan. 7, p. 10.

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Obtaining Electric Energy from Carbon Without Heat. (Ill.) *Elec. World*, Jan 21, p. 28.

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DYNAMOS, SYSTEMS OF DISTRIBUTION, ETC.

The Palmer Dynamo. *Iron.; Lond. Elec. Rev.*, Feb. 10, p 136.

Crompton & Swinburne's Improvements in Dynamo-Electric Machinery. (Ill.) *Lond. Elec. Rev.*, Feb. 3, p. 113. (Corr.) C. E. L. Brown, Feb. 10, p. 151. J. Swinburne, Feb. 17, p. 182,

Kennedy's Compound Wound Dynamo. (Ill.) *Elec. World,* Feb. 4, p. 54.

There is but one field coil. The machine gives 10,850 watts at 620 revs. per min.

Compound Winding. (Corr.) E. Garcke. *Lond. Elec. Rev.*, Feb. 3, p. 128. King, Brown & Co., Feb. 10, p. 151.

Dynamos with Internal Poles. (Ill.) *Lond. Elecn.*, Feb. 3, p. 320.

A Dynamo with Internal Poles. *Indust.; N. Y. Elec. Engr.*, Feb., p. 76.

Note as to successful appeal in Germany against granting a patent to Siemens & Halske for dynamo with internal field magnets.

Automatic Regulation of Dynamo Machines. (Ill.) *Cent. fuer Elek.; Indust.* Feb. 3, p. 123.

Lahmeyer device for making an ordinary shunt wound machine, equivalent to a machine that is over compounded; rise and fall of mercury in a tube varying the resistance of the shunt circuit automatically.

Copper in Dynamos. W. Peukert. *l'Elecn.; Lond. Elec. Engr.* Feb. 10, p. 135; *Zeits. fuer Elek.*

"It will be observed that machines made about three years ago gave 1 watt for every 30 or 45 grams of copper, whereas the machines of Ganz & Co. at the present day give 1 watt for every 1.6 grams of copper."

The Brown Dynamo Electric Machine. (Ill.) *Prac. Elec.*, Feb., p. 155. (A machine for laboratory and school use.)

Winding Armatures and Polarity of Brushes. (Corr.) "C. A. S." *Elec. World*, Feb. 18, p. 81.

Electro-plating Dynamos. (Ill.) Hanson, Van Winkle & Co. *N. Y. Elec. Rev.*, Feb. 4, p. 9.

Electro-plating with the Dynamo. (Peukert.) *N. Y. Elec. Rev.*, Feb. 11, p. 3.

A Simple Method of treating Problems connected with Networks of Conductors. (Ill.) W. E. Sumpner. *Lond. Elec. Engr.*. Feb. 24, p. 175.

Constant Pressure in Electric Town Mains. I. *Lond. Elec. Engr.*, Feb. 3, p. 102. II. Feb. 10, p. 126. III. Feb. 17, p. 150.

Electric Wave and Phase Indicator for Alternating and Undulatory Currents. (Ill.) E. Thomson. *Lond. Elecn.*, Feb. 10, p. 350. *Lond. Elec. Rev.*, Feb. 3, p. 108. (Corr.) p. 127.

On Electro-Dynamometers for Measuring the Mean Intensities of Alternating Currents. G. Maneuvrier and P. Ledeboer. *Comptes Rendus; Lond. Elec. Rev.*, Feb. 17, p. 159.

Phenomena of Retardation in the Induction Coil. Disc'n of paper by W. Stanley, Jr., Am. Inst. Elec. Engrs. *Lond. Elec. Rev.*, Feb. 24, p. 206.

On the Difference of Phases of Currents, on the Retardation of Induction, and on the Dissipation of Energy in Transformers, Experimental and Theoretical Researches. (Ill.) G. Ferraris. *Lond. Elec. Rev.*, Feb. 3, p. 111, Feb. 10, p. 132, Feb. 17, p. 156, Feb. 24, p. 191.

The Co-efficient of Self-Induction. (Corr.) J. A. Fleming. *Lond. Elecn.*, Feb. 17, p. 389.

Formulæ for Converters. G. Forbes. Soc. Tel. Engrs. and Elecns. *Lond. Elecn.*, Feb. 24, p. 408

Transformers. (Ed.) *Lond. Elec. Rev.*, Feb. 24, p. 185.

The Westinghouse System. (Corr.) G. L. Addenbrooke. *Lond. Elec. Rev.*, Feb. 3, p. 126.

Transformer Literature. (Corr.) R. Kennedy. *Lond. Elec. Rev.*, Feb. 24, p. 215.

The Energy of Alternating Currents. O. B. Shallenberger, Nat. Elec. Lt, Asso. *West. Elecn.*, Feb. 25, p. 101.

The Distribution of Electricity by Alternating Currents. T. C. Smith, Nat. Elec. Lt. Asso, *West. Elecn.*, Feb. 25, p. 100.

Electrical Distribution. *Md. Lt. & Ht.*, Feb 2, p. 91.

Notes on Alternate Currents. (Ill.) J. A. Fleming. *Lond. Elecn.*, Feb. 3, p. 314. (Cont'd.) Feb. 24, p. 404.

Discussion on Transformers. Soc. Tel. Engrs. and Elecns. *Indust.*, Feb. 24, p. 198. *Lond. Elecn.*, Feb. 24, p. 419.

The Distribution of Electricity by means of Secondary Generators or Transformers. (Ill.) J. K. D. Mackenzie, Soc. Tel. Engrs. and Elecns. *Lond. Elec. Engr.*, Feb. 17, p. 160; Feb. 24. p. 186. *Indust.*, Feb. 17, p. 173. *Eng.*, Feb. 24, p. 195. *Lond. Elec. Rev.*, Feb. 17, p. 176; Feb. 24, p. 203.

On Alternate Current Transformers with Special Reference to the Best Proportions between Iron and Copper. (Ill.) G. Kapp, Soc. Tel. Engrs. and Elecns. *Lond. Elec. Engr.*, Feb. 10, p. 136, et seq; *Lond. Elecn.*, Feb. 10, p. 338; *Lond. Elec. Rev.*, Feb. 17, p. 170; *Indust.*, Feb. 10, p. 145; *Eng.*, Feb. 17, p. 171, Feb. 24, p. 195.

Cost of Electric Energy. (Note.) *Cent. fur Elek.; Lond. Elec. Engr.*, Feb. 24, p. 171.

ELECTRIC POWER.

The Patten Electric Motor. (Ill.) *Elec. World; Lond. Elecn.*, Feb. 3, p. 325.

A Motor's Performances. (Baxter.) *West. Elecn.*, Feb. 11, p. 65.

Sprague Motors for Mines. (Ill.) *West. Elecn.*, Feb. 18, p. 76.

Plans for an 18-mile motor circuit in the gold-yielding Feather River, Butte County, California; power to be distributed at fourteen points; dynamos driven by turbines.

Electric Motors in a California Gold Mine. *Md. Lt. & Ht.*, Feb. 16, p. 135.

Electric Pumping in Collieries. F. Brain, South Wales Soc. Engrs. *N. Y. Elec. Rev.*, Feb. 18, p. 3.

Electric Transmission of Power in Mines. (Corr.) S. F. Walker. *Eng.*, Feb. 3, p. 114.

Discussion on Electric Power. Inst. Mech. Engrs. *Indust.*, Feb. 10, p. 131.

Losses in Driving Pumps by Electricity. *Indust.; Prac. Elec.*, Feb., p. 162.

Result of the Electric Transmission of Work-Kriegstetten to Solothurn. Report by H. F. Weber. *Lond. Elec. Rev.*, Feb. 17, p. 158.

Electric Transmission of Energy. (Ill.) *N. Y. Elec. Engr.*, Feb., p. 58. Kriegstetten-Solothurn.

The Transmission of Energy Between Solothurn and Kriegstetten. (Corr.) C. E. L. Brown. *Indust.*, Feb. 3, p. 117.

Efficiency Tests of Electric Motors for Amateurs. (Ill.) W. F. D. Crane. *Elec. World*, Feb. 4, p. 50; *Lond. Elec. Engr.*, Feb. 17, p. 153;

An Electric Carriage. (Volk's.) (Ill.) *Lond. Elec'n.; Sci. Am.*, Feb. 11, p. 82.

Electric Boat for French Admiralty. *L'Electricité; Lond. Elec. Engr.* Feb. 17, p. 148.

An accumulator boat with Krebs motor; weight of electrical apparatus 2891 kilos.

Converting a Holtz Machine Into a Motor *Elec. World*, Feb 4, p. 55.

Experiment shown at Cornell by Prof. Nichols, reversing the action of an ordinary Holtz machine and converting it into a motor, the charge being obtained from a larger machine driven by water motor.

Alternating Current Electric Motors. (Ill.) L. Duncan, Am. Inst. Elec. Engrs. *Elec. World*, Feb. 25, p. 97; *Prac. Elec.*, Feb., p. 161; *N. Y. Elec. Rev.*, Feb. 18, p. 11; *Science*, Feb. 24, p. 92.

Electricity as a Motive Power. (Prof. Stroud.) *Prac. Elec.*, Feb., p. 157; *Lond. Elec'n.*, Feb. 3, p. 312.

Electric Motors. G. A. Liebig, Nat. Elec. Lt. Asso. *N. Y. Elec. Rev.*, Feb. 25, p. 10.

Designing Electric Motors. F. Walker. *Lond. Elec. Rev.*, Feb. 24, p. 188.

Motors for Small Powers in Chicago. *West. Elecn.*, Feb. 4, p. 50.

Some Plain Speaking. (Ed.) *Lond. Elec. Engr.*, Feb. 10, p. 132.

Points out that the time has arrived when all tramway companies seeking economy in good faith must resort to electricity, wherever practicable.

The Lineff Electric Tramway. *Elec. World*, Feb. 18, p. 78.

Daft Electric Railway Motors for Pittsburgh. (Ill.) *Elec. World*, Feb. 25, p. 89.

Road has 6-ton motor cars ; 1½ miles in length ; grades up to 16 per cent. ; a perforated band is placed on the grades with which a movable sprocket wheel engages.

Schlesinger Electric Locomotive Motor. (Ill.) *Elec. World*, Feb. 25, p. 88.

The Fisher Electric Railway at Detroit. (Ill.) *Elec. World*, Feb. 4, p. 51. *St. Ry. Jour.*, Feb.

The road has a third rail supported on insulators and placed in a conduit 8 in. sq. with a slot ¾ in. wide, in which the contact wheel travels.

The Electric Railway at Richmond, Va. *Richmond Dispatch; N. Y. Elec. Rev.*, Feb. 11, p. 1.

Sprague Electric Railway in Richmond. (Ill.) *Science*, Feb. 10, p. 68.

Sprague Electric Railway at Richmond. *West. Elecn.*, Feb. 18, p. 75.

The Sprague Electric Railway at Richmond, Va. *Elec. World*, Feb. 11, p. 65.

The Bentley-Knight Electric Railway System. (Ill.) *Elec. World*, Feb. 11, p. 63 ; *Lond. Elec. Engr.*, Feb. 24, p. 182.

The Bentley-Knight Electric Railway System at Alleghony City, Pa. (Ill.) *Elec. World*, Feb. 18, p. 75 ; Feb. 25, p. 92.

The Bentley-Knight Standard Truck. (Ill.) *N. Y. Elec. Rev.*, Feb. 11, p. 9.

Electric Traction—Direct Current Working vs. Accumulators. A. Lineff, Instn. Civ. Engrs. *Lond. Elecn.*, Feb. 24, p. 410.

Accumulators on Tramways. (M. Huber's statistics.) *Lond. Elec. Engr.*, Feb. 10, p. 124.

INSTRUMENTS, MEASUREMENTS, &c.

Thomson Bridge for the Measurement of Small Resistances. (Ill.) *Elek. Zeits.; Elec. World*, Feb. 11, p. 62 ; *Lond. Elecn.*

Siemens and Halske bridge of the Thomson pattern.

Sir Wm. Thomson's New Standard Electrical Measuring Instruments. (Ill.) J. A. Fleming. *Indust.; Elec. World*, Feb. 25, p. 89.

Standardizing Sir W. Thomson's Balances. A. W. Meikle, Glasgow Phys. Soc. *Lond. Elecn.*, Feb. 10, p. 353.

Raworth's Lineman's Galvanometer. (Ill.) *N. Y. Elec. Rev.*, Feb. 4, p. 9.

Eaton's New Ohm-Meter. (Ill.) *Prac. Elec.* Feb., p. 155.

Voltmeter and Controlling Apparatus. (Brueckner, Ross & Co.) (Ill.) *Sci. Am. Supp.*, Feb. 25, p. 10, 131.

On Some Instruments for the Measurement of Electromotive Force and Electrical Power. (Ill.) J. A. Fleming & C. H. Gimingham, Soc. Tel. Engrs. and Elecns.; *Lond. Elec. Engr.*, Feb. 3, p. 116. (Cont'd.)

Portable Voltmeters for Measuring Alternating Potential Differences. (Ill.) Cont'd. W. E. Ayrton and J. Perry, Soc. Tel. Engrs. and Elecns. *Elec. World.* Feb. 4, p. 56, Feb. 11, p. 68.

Irish's Electric Regulator and Meter. (Ill.) *Elec. World*, Feb. 11, p. 66. *West Elecn.*, Feb. 11, p. 62.

A register in which mercury rises and falls, in connection with a series of resistances and clockwork; the mercury being acted upon by the core of a solenoid.

Elementary Testing Suitable for Electric Lighting Systems. (Ill.) T. Gray. *Lond. Elec. Engr.*, Feb. 3, p. 104; Feb. 10, p. 127; Feb. 24, p. 174.

Determination of the Intensity of the Horizontal Component of the Earth's Magnetic Field. T. Gray, Glasgow Univ. Phil, Soc. (Note.) *Lond. Elec. Engr.*, Feb. 17, p. 147.

Apparatus for Determining the Intensity of the Horizontal Component of the Earth's Magnetic Field. T. Gray, Glasgow Univ. Phys. Soc. *Lond. Elecn.*, Feb. 17, p. 391.

Practical Electrical Measurement. J. Swinburne. *Lond. Elec. Rev.; West. Elecn.*, Feb. 4, p. 55.

Electrical Measurements Especially as Applied to Commercial Work. W. A. Anthony, *Jour. Fkln. Inst.; Lond. Elec. Engr.*, Feb. 3, p. 115; Feb. 10, p. 141.

Correct Reading of Needle Instruments. (Ill.) *N. Y. Elec. Rev.*, Feb. 11, p. 7.

Correction for the Length of the Needle of the Tangent Galvanometer. (Ill. Corr.) W. Moon. *Lond. Elec. Rev.*, Feb. 3, p. 127. A. C. Elliott. Feb. 10, p. 162. W. Moon. Feb. 17, p. 182. A. C. Elliott. Feb. 24, p. 215.

Discussion of the Precision of Measurements. S. W. Holman. *Tech. Quarterly; N. Y. Elec. Engr.*, Feb., p. 44.

Watts-Heures or Watts-Heure? (Corr.) R. B. P. *Lond. Elec. Engr.*, Feb. 17, p. 159.

Practical Electrical Units. W. B. Esson. *Lond. Elec. Engr.*, Feb. 17, p. 151 (cont'd).

A New Metre. (Note.) *Lond. Elec. Engr.* Feb. 17, p. 146.

As to the system of measurement which M. Freycinet is seeking to introduce, in substitution for the C. G. S. units.

MISCELLANEOUS.

Wonders of Animal Magnetism. G. de la Tourette. *North Am. Rev.*, Feb., p. 131.

Article read and approved by Prof. Charcot.

A New Thermostat. (Ill.) (Nat. Aut. Fire Alarm Co.) ; *Elec. World*, Feb. 4, p. 52.

The Electro-Engraving Machine. (Ill.) *Elec. World*, Feb. 25, p. 90.

The Electrical Typewriter. (Ill.) F. Higgins. *Lond. Elecn.; Sci. Am.*, Feb. 4, p. 73,

The Wallace Electric Cloth Lining. (Ill.) *West. Elecn.*, Feb. 4, p. 50.

An Electric Matrix Making Machine. *Wood and Iron; West. Elecn.*, Feb. 4, p. 57.

Application of Electricity in the Dental Office. G. W. Whitefield, Chic. Elec. Club. *West. Elecn.*, Feb. 11, p. 67.

A New Electric Furnace. (Cailletet.) *Eng.*, Feb. 17, p. 175.

A hollow cylinder of steel hermetically closed, for conducting experiments on different materials at high temperatures while subjected to great gaseous pressures.

An Electric Chess Recorder. *Indust.; West. Elecn.*, Feb. 4, p. 57; *Sci. Am.*, Feb. 18, p. 97. (Wurstenberger.)

The Induction Top. (Ill.) *La Nature; Elec. World*, Feb. 4, p. 56; *Lond. Elec. Engr.*, Feb. 17, p. 146; *Sci. Am. Supp.*, Feb. 25, p. 10, 130.

An iron disc spun in front of a horseshoe magnet is repelled, and assumes an inclined position which it maintains as long as the velocity is sufficiently great.

Submarine Mining—Electrical Arrangements on Shore. (Ill.) Lt. Col. Bucknill. *Eng.*, Feb. 10, p. 131; Feb. 24, p. 181.

An Improved Mine Exploder. (Ill.) (D. Moseley & Sons.) *Indust.*, Feb. 17, p. 166.

On the Price of the Factor of Safety in Lightning Rods. S. P. Thompson, Phys. Soc. *Lond. Elecn.*, Feb. 3, p. 330; *Elec. World*, Feb. 25, p. 88; *Lond. Elec. Rev.*, Feb. 3, p. 126.

Execution by Electricity. *N. Y. World; West. Elecn.*, Feb. 18, p. 76.

Execution by Electricity. (Ed. Note.) *Md. Lt. & Ht.*, Feb. 9, p. 112.

India Rubber Compensators for Overhead Wires. (Corr.) W. H. Preece; R. S. Bozon. *Lond. Elecn.*, Feb. 3, p. 327, F. Caws. Feb. 10, p. 360.

New Insulated Wires. (Ill.) (Standard Underground Cable Co.) *West. Elecn.*, Feb. 18, p. 80.

A New Weather Proof Line Wire. (Ill.) (Standard Undgd. Cable Co.) *Md. Lt. & Ht.*, Feb. 16, p. 143.

Making Insulated Wires for Electrical Purposes. (Okonite.) (Ill.) *Sci. Am.*, Feb. 25, p. 115.

Insulation. *Md. Lt. & Ht.*, Feb. 16, p. 143.

As to the necessity for better insulation, and the problems to be solved in adapting insulating materials to their work.

Galvanic Belt Frauds. (Corr.) J. Jerritt. *Lond. Elec. Rev.*, Feb. 10, p. 152; E. H. Berry, D. G. Fitzgerald, Feb. 17, p. 182.

Electropathic Belts. (Corr.) J. G. Garson. *Lond. Elec. Rev.*, Feb. 3, p. 128.

The Oldest Pioneer. (W. W. Marks.) *N. Y. Elec. Rev.*, Feb. 18, p. 11.

The Men Who Have Helped Edison to Complete His Inventions. *Sci. Am. Supp.*, Feb. 25, p. 10,131.

Dr. Wollaston. (With portrait.) *Lond. Elec. Engr.*, Feb. 3, p. 109.

Dr. Balfour Stewart, F. R. S. (Obit.) P. G. Tait. *Nature; N. Y. Elec. Engr.*, Feb., p. 64.

Franz Carl Guilleaume. (Obit.) *N. Y. Elec. Engr.*, Feb., p. 77.

Electrical Purification of Sewage. *Prac. Elec.*, Feb., p. 166.

Electricity and Sewage. (Webster.) *West. Elecn.*, Feb. 18, p. 79.

The Electrical Sewage Process. *Lond. Elec. Rev.; Sci. Am.*, Feb. 11, p. 85.

Refining Sugar by Electricity. *N. Y. Elec. Rev.*, Feb. 4, p. 1.

The Paris Exhibition of 1889. (Ed.) *Lond. Elec. Engr.*, Feb. 17, p. 157.

Paris Universal Exhibition of 1889. *Lond. Elec. Engr.*, Feb. 10, p. 130.

The Society of Telegraph Engineers and Electricians and the Paris Exhibition of 1889. (Corr.) F. H. Webb. *Lond. Elecn.*, Feb. 3, p. 327.

Patents and Exhibitions. (Corr.) J. K. Fahie & Son. *Lond. Elec. Engr.*, Feb. 24, p. 18[illegible]; *Lond. Elecn.*, Feb. 24, p. 41[illegible]; *Lond. Elec. Rev.*, Feb. 24, p. 216. Danger of exhibiting unpatented inventions.

The Necessity of a Patent System and the Advantage to be Derived Therefrom. *Md. Lt. & Ht.*, Feb. 2, p. 93.

Improvement in Our Patent Laws. A. Steuart, Nat. Elec. Lt. Asso. *West. Elecn.*, Feb. [illegible], p. [illegible].

The Annual Report of the Commissioner of Patents to Congress. G. H. Stockbridge. *Elec. World*, Feb. 18, p. 76.

The Patent Reform Question once more. G. H. Stockbridge. *Elec. World*, Feb. 4, p. [illegible].

Limitation of U. S. Patents by Foreign Patents. (Corr.) I. H. Davis. *N. Y. Elec. Engr.*, Feb., p. [illegible].

An Amendment to the Patent Law. *Md. Lt. & Ht.*, Feb. 16, p. 136.

Mr. Edison on Patent Protection. (Ed.) *N. Y. Elec. Engr.*, Feb., p. 41. *Sci. Am.*, Feb. [illegible], p. [illegible].

Mr. Edison on the Patent Law. *N. Y. Eve. Post; N. Y. Elec. Engr.*, Feb., p. 6[illegible].

Revision of the Patent Law. C. A. Brown, Chic. Elec. Club; *N. Y. Elec. Engr.*, Feb., p. 6[illegible].

Statistics of United States Patents Relating to Electricity in 1887. T. D. Lockwood. *N. Y. Elec. Engr.*, Feb., p. 40.

1,248 electrical and allied patents; a decrease of 89 from 1886; 6.10 per cent. of all the patents issued.

The Magnetic Properties of Iron. (Ledeboer.) *Eng.*, Feb 17, p. 175.

Magnetization of Iron. *Sci. Am. Supp.*, Feb. 4, p. 10,073.
The possible intensity of magnetic induction.

Wire Space in Large Buildings. C. L. Healy. *Elec. World*, Feb. 25, p. 88.

A Diagram for Electric Wiring. (E. B. E. Stark.) *Eng.*, Feb. 17, p. 175.

The Lightning Rod Question. D. P. Heap. *N. Y. Elec. Rev.; Lond. Elec. Rev.*, Feb. 17, p. 162.

An Improved Electric Bell. (Ill.) (Stocker.) *Sci. Am.* Feb. 25, p. 114.

The New "Hub" Bell. (Ill.) (Elec. Gas Ltg. Co., Boston.) *N. Y. Elec. Rev.* Feb. 11, p. 3.

Sunstroke by Electricity. *Jour. des Debats; West. Elecn.*, Feb. 18, p. 80. *Sci. Am.*, Feb. 18, p. 99.

Conference on Technical Education. *Lond. Elec.*, Feb. 17, p. 391.

Another Electrical Laboratory. (Vienna.) *Lond. Elec. Engr.*, Feb. 24, p. 171.

New Physical Laboratory University of Michigan. (Ill.) *N. Y. Elec. Rev.*, Feb. 18, p. 2; *West. Elecn.*, Feb. 18, p. 77.

The Electric Club of New York. *Eng.*, Feb. 24, p. 195.

Electric Club Memories. *N. Y. Elec. Rev.*, Feb. 11, p. 2.

Opening of the New York Electric Club. (Ill.) *N. Y. Elec. Rev.*, Feb. 4, p. 1. *Md. Lt. & Ht.*, Feb. 9, p. 117

The House Warming of the New York Electric Club. *Md. Lt. & Ht.*, Feb. 2, p. 97.

The Electrical and Magnetic Discoveries of Faraday. H. A. Rowland, N. Y. Elec. Club. *N. Y. Elec. Rev.*, Feb. 4, p. 3. *Md. Lt. & Ht.*, Feb. 9, p. 118.

The Term, "Magnetic Resistance." (Corr.) J. Swinburne. *Lond. Elecn.*, Feb. 24, p. 412.

The Use of the Term "Resistance" in the Description of Physical Phenomena. Phys. Soc. R. H. N. Bosanquet. *Lond. Elecn.*, Feb. 17, p. 390; *Indust.*, Feb. 17, p. 166.

On the Heating Effects of Electrical Currents. W. H. Preece, Royal Soc. *Lond. Elecn*, Feb. 17, p. 386, Feb. 24, p. 411.

Letters for Learners and Unprofessional Readers. (Ill.) H. D. Wilkinson. Chap. VII. *Lond. Elecn.*, Feb. 3, p. 320; Feb. 10, p. 349; Feb. 17, p. 375; Feb. 24, p. 408.

On the Position and Prospects of Electricity as Applied to Engineering. W. Geipel, Instn. Mech. Engrs. *Lond. Elec. Engr.*, Feb. 3, p. 113; Feb. 10, p. 130; Feb. 24, p. 183. *Indust.*, Feb. 10, p. 130; Feb. 17, p. 146. *Eng.*, Feb. 17, p. 172; Feb. 24, p. 202. *Lond. Elecn.*, Feb. 10, p 355; Feb. 24, p. 424.

Copper Production in 1887. *Eng. & Mg. Jour.; N. Y. Elec. Engr.*, Feb., p. 65.—Production in U. S. A., in 1887, 177,200,000 pounds.

Electricity and the Law. *Eng.; N. Y. Elec. Engr.*, Feb., p. 77.

Electric Organ Actions. G. Wacker, Poly. Sec. Am. Inst. *Elec. World*, Feb. 11, p. 62.

Alternating Current Circuits and Fire Alarm Wires. (Corr.) "H. W. F." *Elec. World*, Feb. 11, p. 68.

Effects of Animal Organism on Electro-Magnetic Action. C. H. Murray. *Elec. World*, Feb. 18, p. 74.

Advances the theory that "the magnetism of an electric current is entirely extinguished by the passage of the current through an animal organism."

Electric Welding by Means of the Arc. (Ill.) The Benardos process. *Elec. World*, Feb. 25, p. 91.

Effects of Heating Platinum Wire. (Corr.) "J. H. S." *Elec. World*, Feb. 25, p. 94.

Manufacture of Aluminium. *Prac. Elec.*, Feb., p. 159.

The Meritens Process for Rendering Iron and Steel Inoxidisable. (Note.) *Lond. Elec. Engr.*, Feb. 3, p. 98.

The Institute of Electrical Engineers. (Corr.) S. F. Walker. *Lond. Elec. Engr.*, Feb. 17, p. 150. "Exon," "Electricita," Feb. 24, p. 185.

Admission of ladies to meetings.

Electricity. G. H. Bliss. *West. Elecn.*, Feb. 11, p. 70. Feb. 18, p. 77.

How can we Protect our Watches from the Influence of Magnetism? P. Lange, Nat. Elec. Lt. Asso. *West. Elecn.*, Feb. 25, p. 88.

The Time in Which We Think. *Md. Lt. & Ht.; Sci. Am.*, Feb. 11, p. 81.

Edoux Hydraulic Elevators. (Ill.) *Sci. Am.* Feb. 11, p. 86.

Includes hydro-electrical regulation.

An Improved Electric Circuit Closer. (Stocker.) (Ill.) *Sci. Am.*, Feb. 25, p. 114.

Copper Alloys. (Note.) G. A. Faurie, French Acad. Sci. *Indust.*, Feb. 3, p. 123.

The "Waddington" Electrical Submarine Vessel. (Ill.) *Indust.*, Feb. 17, p. 165.

The Range of Molecular Forces. A. W. Rucker, Lond. Chem. Soc. *Indust.*, Feb. 17, p. 174.

Electric Osmose and Electro-Capillary Action. R. Snowdon, Brit. Asso. Ad. Sci. *Elec. World*, Feb. 4, p. 51; *West. Elecn.*, Feb. 4, p. 56.

On the Effect of Light on the Electric Discharge. E. Wiedemann and H. Ebert, Phys. Med. Soc., Erlangen. *Lond. Elecn.*, Feb. 3, p. 324.

On the Optical Demonstration of Electrical Stress. A. W. Rucker, and C. V. Boys, Phys. Soc. *Lond. Elecn.*, Feb. 3, p. 330.

On a Water-Dropping Influence Machine. S. P. Thompson, Phys. Soc. *Lond. Elecn.*, Feb, 3, p. 330*; Lond. Elec. Rev.*, Feb. 3, p. 126.

Note on the Theory of the Electrophorus. E. C. Rimington. *Lond. Elec. Rev.; West. Elecn.*, Feb. 4, p. 57.

English and American Practice in Electrical Engineering. (Corr.) W. H. Smith. *Indust.*, Feb. 17, p. 170.

The Heating of Electric Discharging Points. (Semmola.) *Eng.*, Feb. 10, p. 148.

Atmospheric Electricity. Palmieri, Acad. Sci., Naples. *Lond. Elec. Engr.*, Feb. 10, p. 125.

Insists that a negative charge of electricity is produced by the evaporation and a positive charge by the condensation of water. The evaporation and condensation must occur with a certain degree of rapidity in order to develop a perceptible electrical effect.

Electrical and Meteorological Observations on the Peak of Teneriffe. R. Abercromby, Meteor. Soc. *Lond. Elec. Eng.*, Feb. 17 p. 149*; Eng.*, Feb. 17, p. 165.

Earth Current Variations. (Note) J. J. Landerer. *Engr.*, Feb 10, p. 148.

Earth Currents and Atmospheric Electricity. (Ed. *West. Elecn.*, Feb. 18, p. 78.

Comments on the official observations to be made in Spain on earth currents and on atmospheric electricity, as proposed at the Congress of 1881.

Observations on Atmospheric Electricity. L. Weber, Brit. Asso. Ad. Sci. *Sci. Am. Supp.*, Feb. 18, p. 10, 114.

Pittsburgh. *Md. Lt. & Ht.*, Feb. 23, p. 164.

George Westinghouse, Jr. *Md. Lt. & Ht.*, Feb. 23, p. 165.

The Standard Underground Cable Company. *Md. Lt. & Ht.*, Feb. 23, p. 167.

Electric "Sunstroke." (Ed. Note.) *Md. Lt. & Ht.*, Feb. 2, p. 90.

The Price of Copper. (Ed. Note.) *Md. Lt. & Ht.*, Feb. 2, p. 91.

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Effect of Current on Wine. (Note.) F. Mengarini. *Jour. Chem. Soc.; Lond, Elec. Engr.*, Feb 24, p. 172.

Current passes through the wine; proportion of alcohol thereby diminished, a perfume imparted to the wine similar to that acquired by maturing; coloring matter is affected, and the wine is put in a better state of preservation.

Action of Sea Water upon Cast Iron. C. N. Draper. *Chem. News; Sci. Am.*, Feb. 4, p. 64.

New Discovered Records of Magnetic Declination. *Science; West. Elecn.*, Feb. 18, p. 81.

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A Soap Battery. A. V. Meserole. *Elec. World*, Feb. 18, p. 78.

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On Ohm's Law in Electrolytes. (Ill.) D. G. Fitzgerald, Brit. Asso. Ad. Sci. *Lond. Elecn.,* Feb. 17, p. 385.

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Nickel Plating Solutions. *Bul. Inter. de l'Elec.; Elec. World,* Feb. 25, p. 94.

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The Storage Battery. *N. Y. Elec. Rev.,* *Sci. Am.,* Feb, 18, p. 102.

Drake and Gorham's Switchboard. (Ill.) *Lond. Elec. Engr.,* Feb. 10, p. 131.

A switchboard designed for accumulator installations.

The Julien Storage Battery. *Prac. Elec.,* Feb., p. 161.

A New Form of Secondary Battery. K. Hedges, Brit. Asso. Ad. Sci. *N. Y. Elec. Engr.,* Feb., p. 59; *Prac. Elec.,* Feb., p. 160.

Drake and Gorham's Excess Indicator. (Ill.) *Lond. Elec. Engr.* Feb. 24, p. 182.

For secondary battery protection; the principle adopted is that of thermic alarms, which depend for their action upon the bending of a compound metallic plate under variations of temperature.

THERMO-ELECTRICITY.

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On the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys With Lead and Tin. E. Von Aubel, Phys. Soc. *Lond. Elecn.,* Feb. 3, p. 330. *Lond. Elec. Rev.,* Feb. 3, p. 125.

The Thermo-Magnetic Properties of Nickel and Iron. (Ill.) Berson. *Ann. de Phys. et de Chimie; Md. Lt. & Ht.,* Feb. 16, p. 138.

The Thermo-Magnetic Properties of Iron and Nickel. H. Tomlinson. *Lond. Elecn.* Feb. 17, p. 383.

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Apparatus operating upon the principle that the magnetic metals lose their magnetic permeability at a certain temperature.

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A New System of Underground Electrical Conductors. (Ill.) D. Brooks. *Lond. Elec. Rev.*, Feb. 3, p. 115. (Corr.) "Conductor," Feb. 10, p. 130. "W.," "F. J.," Feb. 17, p. 182, 183.

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Underground Conductors for Electric Currents. J. M. Smith, Nat. Elec. Lt. Asso. *West. Elecn.*, Feb. 25, p. 103.

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Transmission of Power by Rope. (Ill.) *West. Elecn.*, Feb. 11, p. 64.

Describes a system used by the Chicago Arc Light and Power Company.

Independent Engines in Incandescent Stations. W. L. Church, Nat. Elec. Lt. Asso. *West. Elecn.*, Feb. 25, p. 94; *N. Y. Elec. Rev.*, Feb. 25, p. 9.

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The New Armington-Sims Engine. *Elec. World*, Feb. 11, p. 69.

Gas Engines and Wind Mills for Electric Lighting. *Science*, Feb. 24, p. 93.

Windmills for Generating Electricity. A. R. Wolff. *Stevens' Indicator; Elec. World*, Feb. 4, p. 51.

States that in the United States, on an average, for at least 8 hours out of the 24 of each day, the wind has a velocity exceeding 6 miles per hour, the average velocity during the 8 hours being 16 miles per hour.

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Practical Telephony. (Ill.) Part VI. Thos. D. Lockwood. *West. Elecn.*, March 3, p. 112. Part VII., March 24, p. 146.

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Hawkeye Electric Company. (Ill.) *West. Elecn.*, March 31, p. 159. (System of arc and incandescent lighting.)

Electric Light for Fifth Avenue Stages. *N. Y. Elec. Rev.*, March 10, p. 8.

A Most Creditable Electric Light Installation. *N. Y. Elec. Rev.*, March 10, p. 1.

U. S. Court House and Post Office, Philadelphia.

Electricity in the Hotel Ponce de Leon, St. Augustine. H. B. Rockwood. *Sci. Am.*, March 31, p. 193.

Power is obtained from driven wells, for a large Edison installation.

Minor Phenomena of the Incandescent Electric Lamp. *Sci. Am.*, March 10, p. 144; *Lond. Elecn.*, March 23, p. 544.

The Edison Philadelphia Plant. (Ill.) *N. Y. Elec. Rev.*, March 17, p. 8. (Views of new station.)

The Electric Light Plant in the Equitable Building, N. Y. (Ill.) *N. Y. Elec. Rev.*, March 17, p. 6.

The largest isolated incandescent plant in the world; the building is wired for between 6000 and 7000 lights, and there are seven 600-light dynamos with a common field exciter.

The Electric Light for Elevated Cars. *N. Y. Sun; N. Y. Elec. Rev.*, March 17, p. 2.

On the Cost of Central Station Lighting. R. E. Crompton. *Indust.*, March 16, p. 276. (Corr.) C. W. S. Crawley. J. K. D. Mackenzie. March 23, p. 300.

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Central Station Lighting by Storage Batteries. *Indust.*, March 2, p. 220; *N. Y. Elec. Rev.*, March 24, p. 10.

Artistic Electric Lighting. H. Swan. City and Guilds of London Inst., Old Students Meeting. *Lond. Elec. Engr.*, March 9, p. 224; *West. Elecn.*, March 31, p. 163.

Portable Reading Lamp. *Lond. Elec. Engr.*, March 2, p. 211.

The Seel Incandescent Lamp. *West. Elecn.*, March 3, p. 112.

An Electrically Lighted Coupe. *West. Elecn.*, March 3, p. 109.

Another Lamp Freak. (Ill.) *Elec. World*, March 31, p. 162.

An Edison "municipal" lamp showing two well marked distentions of the globe, caused by failure of the filament. In the case previously noted, the glass, on becoming soft, had yielded to the pressure of the outer air and had been forced in.

Incandescent Lamps. *Electrician; Elec. World*, March 24, p. 150.

Why do Incandescent Lamps Blacken? Hosea E. Husted. *Elec. World*, March 17, p. 148.

How to Determine the Nature of the Current in Incandescent Lighting. *Lond. Elecn.; Elec. World,* March 17, p. 135.

Electric Safety Lamps. *Elec. World,* March 10, p. 126.

The Heisler Series Incandescent System. (Ill.) *Elec. World,* March 10, p. 123.

Plant at Matteawan, N. Y., with one machine furnishing 190 lights of 20 and 30 c. p. on 18 miles of circuits, No. 8 B. & S. wire; all lamps in series.

New Lampholder. (Note.) *Lond. Elec. Rev.,* March 30, p. 346.

The "Municipal" Lamp. (Note.) *Lond. Elec. Rev.,* March 9, p. 263.

Note on the Efficiency of Incandescent Lamps with Direct and Alternating Currents. W. E. Ayrton & J. Perry, Phys. Soc. *Lond. Elecn.,* March 2, p. 454; *Lond. Elec. Rev.,* March 9, p. 277; *Eng.,* March 2, p. 223; *Eng.,* March 16, p. 272; *Indust.,* March 2, p. 214.

Efficiency of Incandescent Lamps and Electric Traction. (Corr.) W. E. Ayrton. *Lond. Elecn.,* March 9, p. 481.

Vesey's Electrical Odontoscope. (Ill.) *Lond. Elecn.,* March 2, p. 438

The Elements of Electric Lighting. Philip Atkinson. No. XXXI. *West. Elecn.,* March 3. p. 115; No. XXXII., March 10, p. 124; No. XXXIII., March 17, p. 134.

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Electric Lighting. (Ed.) *Lond. Elec. Rev.,* March 9, p. 249.

The Electric Lighting Act. (1882) Amendment Bill. *Lond. Elecn.,* March 9, p. 484; *Lond. Elec. Rev.,* March 9, p. 259; *Lond. Elec. Engr.,* March 9, p. 235.

The Electric Light Act. (Note.) *Lond. Elec. Rev.,* March 30, p. 343. *Indust.,* March 2, p. 246.

Tne Electric Light and our Eyesight. *Rev. Int. de L'Electricité; Md. Lt. & Ht.,* March 22, p. 332.

Contracts for Electric Light Wiring. *Lond. Elec. Rev.; Elec. World,* March 17, p. 135.

The Accumulation of Dust on Electric Light Wires. Geo. A. Nellis. *Elec. World,* March 10, p. 122.

The statement is made that the wires indoor carrying alternating current do not blacken or accumulate dust, whereas those carrying direct current always do.

Safety Fuses for Electric Light Circuits. *Elec. World,* March 10, p. 122.

Safety Fuses. (Corr.) "Electrophile." R. Fred. Yorke. *Lond. Elec. Rev.,* March 2, p. 246.

Putting up Internal Work. G. L. Addenbrooke. *Lond. Elec. Rev.,* March 30, p. 334.

Insulation for Conductors for Internal Work. (Note.) G. L. Addenbrooke. "Anne Insulator." *Lond. Elec. Rev.,* March 16, p. 304.

Numbering Conductors, *Lond. Elecn.*, March 16, p. 507.
System used by the Anglo-Am. Brush E. L. Corporation.

Gas into Electricity, *Prac. Elec.*, March, p. 187.

The Relative Cost of Gas, Oil and Electricity. (Note.) *Eng.*, March 23, p. 297.

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A New Dynamo. *N. Y. Elec. Rev.*, March 17, p. 8. (J. J. Wood.)

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Eickemeyer's Dynamo. (Ill.) *Lond. Elec. Rev.*, March 23, p. 310; *N. Y. Elec. Engr.*, Mach, p. 84. (The "Ironclad.")

The Crompton-Swinburne Dynamo. (Corr.) *Lond. Elec. Rev.*, March 2, p. 247.

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A patented system for obtaining an alternating low tension current from a direct high tension current.

Current Distribution of the Near Future. Dr. Louis Duncan. *Lond. Elec. Engr.*, March 2, p. 207.

Notes on Alternate Currents. J. A. Fleming (continued from p. 406). (Ill.) *Lond. Elecn.*, March 16, p. 502.

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The Measurement of the Power Supplied to the Primary Coil of a Transformer. (Ill.) E. C. Rimington. Phys. Soc. *Lond. Elecn.*, March 23, p. 554; *Eng.*, March 16, p. 271.

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The Counter-Electro-Motive Force of Transformers, or Converters. *Md. Lt. & Ht.*, March 15, p. 302.

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The Comparative Value of the Continuous and the Alternating Current Systems for the Commercial Distribution of Electricity. (Ill.) H. Ward Leonard, Chic. Elec. Club. *Elec. World*, March 24, p. 152; *Prac. Elec.*, March, p. 188.

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On Alternate Current Transformers, with Special Reference to the Best Proportion between Iron and Copper. (Ill.) Gisbert Kapp, Soc. Tel. Engrs. and Elecns. *Elec. World*, March 17, p. 139.

The Distribution of Electricity by Means of Secondary Generators or Transformers. J. K. D. Mackenzie, Soc. Tel. Engrs. and Elecns. *Elec. World*, March 24, p. 154; *West. Elecn.*, March 10, p. 122; March 17, p. 135.

Alternate Current Transformers. (Ill.) *Elec. World*, March 24, p. 153. Discussion of the Kapp and Mackenzie papers before the Soc. Tel. Engrs. and Elecns.

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ELECTRIC POWER.

The Wells Machine for Cleaning Horses. (Ill.) *N. Y. Elec. Rev.* March 10, p. 10. (Showing use of Sprague Motor.)

Electric Transmission of Power. *Sci. Am. Supp.*, March 3, p. 10,142. The Kriegstetten and Solothurn installation and results of report thereon, with tables of results.

Fire Engines Operated by Electricity. *Jewelry News*; *Sci. Am.*, March 3, p. 137.

Simple Electric Motor. (Ill.) Geo. M. Hopkins. *Sci. Am.*, March 17, p. 165. *Lond. Elec. Engr.*, March 30, p. 296.

Electric Motors. F. L. Pope. *Scribner's Mag.*; *Lond. Elec. Engr.*, March 23, p. 281.

Glass Cutting by Electricity. (Note.) *Lond. Elec. Rev.*, March 30, p. 345.

Pumping in Mines by Electricity. *Lond. Elec. Rev.*, March 30, p. 33 *Lond. Elecn.*, March 30, p. 565; *Lond. Elec. Engr.*, March 30, p. 29 *Indust.*, March 30, p. 326.

Electric Motors. G. A. Liebig, Jr., Nat. Elec. Lt. Assn. *Md. Lt. & Ht* March 1, p. 203; *Elec. World*, March 3, p. 106; *N. Y. Elec. Engr.*, Marc p. 105; *N. Y. Elec. Rev.*, March 3, p. 9; *Lond. Elec. Engr.*, March 1 p. 254.

New C. & C. Shunt Wound Motor. (Ill.) *Elec. World*, March 10, p. 125

The "Belding" Motor. (Ill.) *Elec, World*, March 10, p. 127.

Electric Motors for Organs and Launches. (Ill.) *Elec. World* March 17, p. 137. (Shows use of small "C. & C." motors.)

Reversing Electric Motors. (Corr.) *Elec. World*, March 24, p. 154.

Transmission of Power in Madrid. *El Economista; Elec. World*, March 17, p. 137; *Lond. Elec. Rev.*, March 2, p. 231.

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The Efficiency of Small Electric Motors. E. H. Clifford. Abstract of Paper from *Proceedings* of Am. Academy Arts and Sciences. *Jour. Ins. Civ. Engrs.; Lond. Elec. Engr.*, March 16, p. 250.

Alternating Current Electro-Motors. Dr. Louis Duncan, Am. Ins. Elec. Engrs. *Lond. Elecn.*, March 9, p. 471; *Md. Lt. & Ht.*, March 8, p. 280; *N. Y. Elec. Engr.*, March, pp. 85, 121; *Lond. Elec. Engr.*, March 9, p. 231; March 16, p. 257.

Electric Tramways. R. W. Blackwell. *St. Ry. Jour.*, March, p. 70.

The Bentley-Knight Electric Railway. (Ill.) *St. Ry. Jour.*, March, p. 63.

The Bentley-Knight System at Allegheny, Pa. *N. Y. Elec. Rev.*, March 3, p. 1.

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Electric Traction. (Ed.) *Lond. Elec. Rev.*, March 30, p. 329. (Note.) Edward Manville. March 23, p. 326.

Schlesinger Electric Locomotive Motor. (Ill.) *Lond. Elec. Engr.*, March 9, p. 222.

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The Hannah's Elevated Railway. (Ill.) *West. Elecn.*, March 3, p. 110.

New Switch on the Julien Electric Car. (Ill.) *Elec. World*, March 31, p. 161.

The Frankfurt-Offenbach Electric Tramway. (Ill.) E. Krause. *Lond. Elecn.*, March 23, p. 533.

An admirably full and detailed description of the road, which is operated on the overhead system with slotted tube conductors.

Electric Traction—Closed Conduit Systems. (Corr.) W. Geipel. *Lond Elecn.*, March 9, p. 482. Lond. Ins. Civ. Engrs. *West. Elecn.*, March 31, p. 164.

Electric Tramways with Stationary Metallic Conductors. R. W. Blackwell. *Lond. Elec. Engr.*, March 23, p. 279.

Electrical Street Railways. (Corr.) "Engineer." *N. Y. Elec Rev.*, March 3, p. 10.

Underground Haulage by Electricity. (Ill.) *Sci. Am. Supp.*, March 31, p. 10,211.

Wm. Geipel's paper on application of electric railways to mine work.

The Electro-Automatic Transit Company of Baltimore. (Ill.) *N. Y. Elec. Rev.*, March 10, p. 8.

For carrying packages across the country by electric express.

INSTRUMENTS, MEASUREMENTS, &c.

Elementary Testing Suitable for Electric Lighting Systems. (Ill.) Thomas Gray. *Lond. Elec. Engr.*, (cont'd from p. 129,) March 2, p. 199; March 16, p. 246; March 30, p. 294.

Letters for Learners and Unprofessional Readers. (Ill.) H. D. Wilkinson. *Lond. Elecn.*, March 2, p. 438; March 9, p. 477; March 16, p. 504; March 23, p. 543; March 30, p. 566. Chap. VIII. The Calibration of Current Indicators.

"Le Wattmetre et son Maniement." (Review.) *Lond. Elecn.*, March 16, p. 506.

"The Percentage Accuracy of Wattmeters." (Corr.) L. R. Wilberforce. March 30, p. 580.

Instruments of Precision of the Mather Electric Company. (Ill.) *Elec. World*, March 24, p. 149.

New resistance box, bridge and galvanometer devised by Prof. W. A. Anthony.

The Last Model of the Aron Electric Motor. *Sci. Am. Supp.*, March 10, p. 10,160.

Western Electric Co's Electric Current Indicator. (Ill.) *N. Y. Elec. Rev.*, March 3, p. 1.

The Sesemann Ammeter. (Note.) *Lond. Elec. Engr.*, March 2, p. 195.

Application of the Deci-Ampere or Centi-Ampere Balance to the Determination of the E. M. F. of Voltaic Cells. *N. Y. Elec. Rev.*, March 31, p. 1.

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Portable Voltmeters for Measuring Alternating Potential Differences. W. E. Ayrton and John Perry, Soc. Tel. Engrs. and Elecns. (Concluded.) *Elec. World*, March 10, p. 128.

A New Pattern of Reflecting Galvanometer with Lamp and Scale. (Ill.) G. L. Addenbrooke, Phys. Soc. *Lond. Elec. Rev.*, March 16, p. 289; *Lond. Elecn.*, March 16, p. 517; *Eng.*, March 16, p. 271; *Indust.*, March 23, p. 304.

Modern Telegraph Galvanometers. W. Moon. *Lond. Elec. Rev.*, March 9, p. 253.

A Mechanical Temperature Compensation for the Bifilar Magnetometer. "G. W. T." *Lond. Elecn.*, March 23, p. 545.

The Unit of Resistance. (Corr.) "E. R. R." *Elec. World*, March 31, p. 168.

Practical Electrical Units. W. B. Esson. (Cont'd from p. 153.) *Lond. Elec. Engr.*, March 9, p. 222.

Queries as to Calculations. (Corr.) H. E. *Elec. World*, March 10, p. 122.

The Notation of Magnetic Quantities. (Corr.) E. Hospitalier. *Lond. Elecn.*, March 2, p. 451.

Wire Tables. *Md. Lt. & Ht.*, March 1, p. 177.

Harcourt's New Pentane Standard Lamp. (Ill.) *Lond. Elecn.*, March 2, p. 456; *Lond. Elec. Engr.*, March 2, p. 212; *Elec. World*, March 10, p. 127.

Von Hefner Alteneck's Amyl Acetate Standard of Light. *Elek. Zeits.; N. Y. Elec. Rev.* March 31, p. 10.

Standards of Length and their Practical Application.. Geo. M. Bond, Soc. of Arts. *Md. Lt. & Ht.*, March 22, p. 329; March 29, p. 354.

The Radio Micrometer. (Note.) *N. Y. Elec. Rev.*, March 24, p. 5.

UNDERGROUND WIRES.

Underground Conductors. (Ed.) *N. Y. Elec. Engr.; Lond. Elec. Rev.* March 23, p. 305.

The Brooks System. (Note.) *Lond. Elec. Rev.*, March 23, p. 327. David Brooks. (Ill.) p. 328.

The Undergrounding of Electric Arc Light Wires. W. W. Leggett, Nat. Elec. Lt. Assn. *Md. Lt. & Ht.*, March 8, p. 272; *N. Y. Elec. Engr.*, March, p. 91; *N. Y. Elec. Rev.*, March 3, pp. 4, 8; *Lond. Elec. Engr.*, March 30, p. 304; *Elec. World*, March 3, p. 115.

Underground Conductors for Electric Currents. Nat. Elec. Lt. Assn. Jesse M. Smith. *Elec. World*, March 3, p. 116; *Md. Lt. & Ht.*, March 1, p. 224; *N. Y. Elec. Engr.*, March, p. 94; *N. Y. Elec. Rev.*, March 3, p. 10.

Methods of Protecting Ends and Splices of Underground Cables. (Ill.) W. R. Patterson. *West. Elecn.*, March 10, p. 121.

Underground Wires for Boston. *Md. Lt. & Ht.*, March 22, p. 326. Public hearing. Testimony of experts.

Underground Electric Light Wires. *Elec. World; Indust.*, March 16, p. 272. (Corr.) Frank Brewer. March 23, p. 300; March 30, p. 330.

THERMO-ELECTRICITY.

E. Raub's New Thermo-Electric Battery. G. Betz. *Lond. Elec. Rev.*, March 30, p. 332.

The Temperature at which Nickel Begins Suddenly to Lose its Magnetic Properties. (Corr.) Herbert Tomlinson. *Lond. Elecn.*, March 16, p. 520; *Lond. Elec. Rev.*, March 9, p. 277. (Phys. Soc.) *Eng.*, March 2, p. 223.

Thermo-Magnetic Motor. (Ill.) *Sci. Am. Supp.*, March 10, p. 10,160. W. B. Cooper, Inventor.

On Some Early Forms of Electric Furnaces. (No. 2, Depretz's Electric Furnaces.) Edwin J. Houston. *Jour. Fkln. Inst.*, Feb., p. 127.

MECHANICAL, &c.

Compound "Roboy" Engine. (Ill.) *Lond. Elec. Engr.*, March 2, p. 201.

A New Boiler Setting. (Ill.) *Md. Lt. & Ht.*, March 15, p. 305. The Weitmyer.

Boston Heating System. A. V. Abbott, Boston Elec. Club. *Md. Lt. & Ht.*, March 1, p. 242.

A Planer for Dynamo Builders. (Ill.) *West. Elecn.*, March 3, p. 112.

"Economy Trials of a Non-Condensing Steam Engine—Simple Compound and Triple." P. W. Willans, Ins. Civ. Engrs. *Lond. Elec. Engr.*, March 23, p. 280.

Leather Belting: Its Origin and Progress. (Ill.) Chas. A. Schieren, Nat. Elec. Lt. Assn. *Md. Lt. & Ht.*, March 8, pp. 275–279. *N. Y. Elec. Engr.*, March, p. 112; *N. Y. Elec. Rev.*, March 3, p. 2; *Elec. World*, March 3, p. 105.

A New Belt Controller for Dynamos. (Ill.) *Elec. World*, March 10, p. 127.

Independent Engines for Incandescent Light Stations. (Ill.) W. L. Church, Nat. Elec. Lt. Assn. *Elec. World*, March 3, p. 108; *Lond. Elecn.*, March 16, p. 513; *Md. Lt. & Ht.*, March 1, p. 208; March 15, p. 306; *N. Y. Elec. Engr.*, March, p. 97; *N. Y. Elec. Rev.*, March 3, p. 16; *Lond. Elec. Engr.*, March 16, p. 248; March 23, p. 282.

Economic Value of Steam Pressure Records. J. B. Edson, Nat. Elec. Lt. Assn. *Elec. World*, March 3, p. 110; *N. Y. Elec. Engr.*, March, p. 109; *N. Y. Elec. Rev.*, March 10, p. 5.

Paillard's Non-Magnetic Compensating Balance and Hair-Spring for Watches. Prof. Edwin J. Houston. *Prac. Elec.*, March, p. 179. (Ill.) *Jour. Fkln. Inst.*

Paillard's Non-Magnetic Compensating Balance and Hair-Spring for Watches. Edwin J. Houston. *Jour. Fkln. Inst.*, March, p. 238.

Gas Engines for Electric Plants. (Ill.) *Prac. Elec.*, March, p. 182. The Körting engine.

BATTERIES, ELECTROLYSIS, &c.

Improved Primary Battery. (Ill.) *Sci. Am. Supp.*, March 17, p. 10175. (P. Selby.)

The Schanschieff Primary Battery. *Elec. World*, March 31, p. 167.

The New "Law" Battery. (Ill.) *Elec. World*, March 31, p. 168.

A Novel Form of Primary Battery. (Ill.) *Elec. World*, March 31, p. 168.

The Woodward battery, in one form of which the positive electrode consists of zinc sponge, and the negative consists of a piece of iron, around which is placed a jacket of perforated iron, the space between the casing and the piece being filled with oxide of copper and iron filings or turnings. The electrode is placed in a caustic potash solution.

Oxide of Copper Battery for Incandescent Lighting. (Corr) "H. M." *Elec. World*, March 24, p. 148.

Partz's Depolarizing Compound. *Elec. World*, March 17, p. 138.

Magnesium as a Positive Element. *Lond. Elecn.*, March 16, p. 512.

The Electrolysis of Copper Sulphate in Standardizing Electrical Instruments. A. W. Meikle. (Ill.) *Lond. Elec. Rev.*, March 23, p. 315; March 30, p. 335; *Lond. Elecn.*, March 30, p. 571.

Raworth's Battery and Detector. *West. Elecn.*, March 31, p. 158.

Combined Battery and Detector. (Note.) *Lond. Elec. Rev.*, March 9, p. 263.

Development of Voltaic Electricity by Atmospheric Oxidation. *N. Y. Elec. Rev.*, March 24, p. 5.

Electrolytic Disinfection of Crude Alcohols. *Lond. Elec. Engr.*, March 9, p. 230.

Electrolysis. *Elec. World*, March 31, p. 160.

The Electrolytic Pole Indicator. (Ill.) Note. *Lond. Elec. Rev.* March 9, p. 263; *West. Elecn.*, 31, p. 158.

Watt's Electrolytic Zinc Process. *Lond. Elec. Rev.*, March 9, p. 258; *West. Elecn.*, March 31, p. 157.

Experiments on Electrolysis. W. W. Haldane Gee, H. Holden and C. H. Lees. (Phys. Soc.) *Lond. Elec. Rev.*, March 9, p. 277; *Eng.*, March 2, p. 223.

Electrolytically-Formed Copper Pipes. *Eng.; Lond. Elecn.*, March 2, p. 436.

Producing Oxygen Gas by Electrolysis. B. W. Smith. *Lond. Elecn.*, March 30, p. 581.

Dujardin's Improvements in Accumulators. (Ill.) *N. Y. Elec. Rev.*, March 24, p. 1; *Science*, March 30, p. 154.

The New Gibson Storage Battery. (Ill.) *Elec. World*, March 31, p. 162.

Exemplifies process for the retention of the active material in capsules rolled and clinched into the plates.

The Carrière Accumulator. *Elec. World*, March 24, p. 150.

Carbon plates having grooved or corrugated surface coated with a composition made by mixing litharge with dilute sulphuric acid containing one-tenth of the strong acid.

Starting the Dynamo On to the Accumulators. (Ill. Note.) T. W. W. Melhuish. *Lond. Elecn*, March 2, p. 451.

Experiments with Accumulators. *Elek. Zeits.; N. Y. Elec. Rev.*, March 31, p. 1.

Results with Huber accumulators, which gave an efficiency in ampere hours of 90.7 per cent., or, in total electrical work, 78.4 per cent.

Secondary Batteries. *Science*, March 2, p. 104.

MISCELLANEOUS.

Improved Electric Bell. (Note.) *Lond. Elec. Rev.*, March 23, p. 322.

Electric Bells. *Lond. Elecn.*, March 23, p. 531.

Dr. S. P. Thompson at Royal Vic. Hall.

Electric Current in the Mouth. *West. Elecn.*, March 3, p. 110.

Electricity and Dentistry. *N. Y. Elec. Rev.*, March 3, p. 1.

Electrical Treatment of Sewage. *Science*, March 23, p. 141.

Electrical and Magnetic Discoveries of Faraday. Prof. Henry A. Rowland. (Cont'd.) *El. Lt. & Ht.*, March 1, p. 248; March 15, p. 303.

Electricity as a Disinfectant of Sewage. *Lond. Standard; N. Y. Elec. Rev.*, March 10, p. 10.

The Optical Demonstration of Electrical Stress. *Elec. World*, March 10, p. 123. (Rucker and Boys' experiments before the Physical Society.)

Electrical Method of Rendering Visible the Form of Vibrating Strings. *Elec. World*, March 10, p. 123. (Puluj.)

Tables to Facilitate the Calculation of Strains of Overhead Line Wires. (Ill.) *Lond. Elec. Rev.*, March 30, p. 337.

The Electric Club. (Ill.) *Sci. Am.*, March 3, p. 127; *N. Y. Elec. Rev.*, March 10, p. 1; *Eng.; N. Y. Elec. Rev.*, March 17, p. 9.

Recent Developments of Electricity. (Ill.) *Elec. World*, March 10, p. 126. (Portion of article by S. S. Wheeler in *Harper's Weekly.*)

Recent Developments of Electricity as an Industrial Art. S. S. Wheeler. *Harper's Weekly*, Feb. 28.

Submarine Mining. Lieut.-Col. Bucknill. (Ill.) *Eng.*, March 9, p. 230. March 23, p. 281.

The Firing Station. The Store Depôt.

Firing High Explosives by Means of Electricity. *Elec. World*, March 17, p. 136.

Electrical Appliances Used With Torpedoes and Submarine Mines. (Ill.) Capt. C. A. McAvoy. *Lond. Elecn.*, March 9, p. 469; March 16, p 501.

A New Coupling for Joining Wires. (Ill.) *Elec. World*, March 31, p. 167.

An ingenious "vice-grip" split tube coupling devised by Carl Hering.

The Simplex Tree Insulator. (Ill.) *N. Y. Elec. Rev.*, March 10, p. 1.

Equipment of the Paris International Electric Laboratory. *West. Elecn.*, March 31, p. 165.

The Paris Electrical Laboratory. *Indust.*, March 2, p. 224; *Science*, March 30, p. 154.

The Working of Metals by Electric Power. *Prac. Elec.*, March, p. 188,

Tests of Electric Welding. *Am. Eng. Rec.; Lond. Elecn.*, March 2, p. 434.

Electrical Welding. (Ill.) *Sci. Am. Supp.*, March 3, p. 10,144; *Sci. Am.*, March 24, p. 185.

The Benardos system of welding by the electric arc.

On the Heating Effects of Electric Currents. *Lond. Elec. Rev.*, March 9, p. 258.

The Heating of Wires by Electric Currents. (Note.) *Indust.*, March 16, p. 279.

The Protection of Buildings from Lightning. *Indust.*, March 23, p. 301.

Atmospheric Electricity. Dr. W. Marcet, Royal Met, Soc. *Eng.*, March 30, p. 324.

Atmospheric Electricity. Dr. W. Marcet. Non-existence of Thunderbolts. G. J. Symons, Royal Met. Soc. *Lond. Elec. Engr.*, March 23, p. 284.

The Non-Existence of Thunderbolts. G. J. Symons, Royal Met. Soc. *Lond. Elecn.*, March 23, p. 545.

Electrostatic Attraction in Liquids. *Elec. World*, March 24, p. 148.

The Influence of Light upon the Electrostatic Charge. *Lond. Elecn.*, March 30, p. 578.

On the Effect of Light on the Electric Discharge. E. Wiedemann and H. Ebert. *Elec. World*, March 10, p. 123; *Lond. Elec. Engr.*, March 23, p. 275.

Patents as Investments. *Sci. Am.*, March 31, p. 201.

Patent Legislation. Nat. Elec. Lt. Assn. *N. Y. Elec. Rev.*, March 10, p. 2. (Discussion at Pittsburgh, Feb. 22.)

The Past Year's Work of the Patent Office. *N. Y. Elec. Rev.*, March 10, p. 7.

Switch-Boards. (Ill.) *Lond. Elecn.*, March 2, p. 455.

Our Electrical Defences. *Lond. Elecn.*, March 2, p. 440.

High Resistance Metals and Alloys. W. M. Mordey. *Lond. Elecn.*, March 30, p. 564.

Note on the Specific Electrical Resistance of Hatfield's Non-Magnetic Manganese Steel Wire. Prof. J. A. Fleming. *Lond. Elecn.*, March 9, p. 479.

On the Changes Produced by Magnetization in the Dimensions of Rings and Rods of Iron and of some other Metals. Shelford Bidwell, Royal Soc. *Lond. Elec. Rev.*, March 9, p. 270: March 23, p. 538.

Electrical Stress. Soc. Tel. Eng. and Elecns. *Lond. Elec. Rev.*, March 30, p. 348; *Lond. Elecn.*, March 2, p. 441; (Ill.) March 9, p. 486. (Corr.) E. Manville. March 23, p. 524; *Eng.*, March 30, p. 316.

Kirchoff's Laws and their Application. (Ill.) E. C. Rimington. *Lond. Elec. Rev.*, March 2, p. 222; 9, p. 253; 16, p. 285; 23, p. 308; 30, p. 340.

Berliner's Molecularium. (Ill.) *Elec. World; Lond. Elec. Rev.*, March 9, p. 255.

Snowstorms. (Corr.) Celt. *Lond. Elec. Rev.*, March 2, p. 248.

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An interesting experiment at Southampton, England, in consuming "town refuse," the heat thus generated to be used under the boilers of an electric lighting plant.

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An Electric Ore Tester. (Ill.) *Elec. World*, April 21, p. 198. Telephonic apparatus having a brush in circuit, which as it passes over ore, ground with metal in it, is supposed to give a click in the telephone at every metallic contact.

The Caloriphone. Léon de Pontois. *Elec. World*, April 7, p. 175.

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A very interesting and significant statement of the manner in which electric light stations can be operated at a loss, without effort.

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A New Form of Induction Coil. (Ill.) *Elec. World*, April 28, p. 214 (Elias E. Ries.)

In this type of transformer the high tension primary wire is completely in closed within and protected by the low tension secondary, the two forming single compound conductor.

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A meter invented by Prof. Elihu Thomson, in which the expansion of the liquid, such as alcohol, in two bulbs, by the heating of coils in them, causes the bulbs to oscillate, and thus to register.

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On Some Early Forms of Electric Furnaces. Edwin J. Houston. *Lond. Elec. Rev.*, April 6, p. 365. (Depretz's and Lontin's electric furnaces.) *Jour. Fkln. Inst.*, April, p. 300.

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Comprises elements in annular shape, with a special "refrigerating" device for the forcible introduction of air.

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A Lightning Rod Fallacy. *Building; Md. Lt. & Ht.*, April 26, p. 461.

A correction of some lightning rod fallacies, and as to the necessity of a large ground plate, well sunken in moist ground.

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A New Frictional Machine. (Note.) *Lond. Elec. Rev.*, April 27, p. 448. (Tudsbury.)

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An instrument to secure the setting of pole arms at exact right angles.

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Metallic bracelets and leglets for shunting a part of the current.

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Image obtained, for example, when a piece of glass separates a sheet of tin foil and a coin, and the metals are connected to a frictional machine.

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Some Telegraph History. *Century Mag.; N. Y. Elec. Rev.*, May 5, p. 6. (Alfred Vail.)

Co-operation Among Telegraphers. T. C. Martin. *Elec. World*, May 12, p. 243.

Gives statistics as to the saving and disbursement of about $500,000 among operators in various insurance and co-operative movements.

Western Union Figures. *West. Elecn.*, May 5, p. 220.

TELEPHONY.

An Improved Apparatus for Examining Ores. (Ill.) *Sci. Am.*, May 12, p. 291; *Lond. Elec. Engr.*, May 25, p. 482.

Use of the telephone in connection with metallic brushes or fingers.

Instrument for Testing Telephone Transmitters and Induction Coils. (Ill.) *Elec. World*, May 12, p. 242. (Hammond V. Hayes.)

Apparatus based on the principle of the Hughes induction balance.

Practical Telephony. T. D. Lockwood. *West. Elecn.*, May 12, p. 240. (Cont'd).

The Use of a Dynamo in Telephone Exchanges. (Ill.) *N. Y. Elec. Rev.*, May 19, p. 7. (Dyer.)

Experiments on the Blake Contact. (Ill.) *N. Y. Elec. Rev.*, May 5, p. 8. (Patterson, Am. Academy Arts and Sciences.)

The Telephone in Wheatstone Bridge Measurements. (Corr.) C. H. Richardson. *N. Y. Elec. Rev.*, May 5, p. 7; May 19, p. 6.

The Stocker Box Telephone. (Ill.) *Lond. Elec. Engr.*, May 11, p. 448.

Long-Distance Telephony. (Corr.) J. O. Fry, *Lond. Elecn.*, May 4, p. 762. (Corr.) J. O. Fry. *Lond. Elec. Engr.*, May 4, p. 418. *Lond. Elec. Rev.*, May 4, p. 493.

The Water-Jet Telephone Transmitter. *Lond. Elecn.*, May 18, p. 45.

The Measurement by the Telephone of Residences Not Free from Induction. *Lond. Elec. Rev.*, May 18, p. 536.

The Rise and Progress of Telephony in the United Kingdom. (Continued from p. 689.) *Lond. Elecn.*, May 18, p. 41. (Corr.) May 25, p. 95.

Telephones in the River Plate. *Lond. Elecn.*, May 18, p. 43.

Another Important Telephone Decision. *N. Y. Elec. Engr.*, May p. 231
(Am. Bell Tel. Co. vs. Southern Tel. Co.)

The Telephone Decision Interpreted. *N. Y. Elec. Rev.*, May 5, p. 1. (Judge Brewer.)

Judgment of the Supreme Court of the United States in the Telephone Appeals. (Cont'd from p. 668.) *Lond. Elecn.*, May 4, p. 757.

The Graphophone in Congress. *Mod. Lt. & Ht.*, May 3, p. 484.

The Phonograph. (Ill.) E. T. Gilliland, N. Y. Elec. Club. *Elec. World*, May 19, p. 258; *Mod. Lt. & Ht.*, May 17, p. 520; *West. Elecn.*, May 19, p. 245; *N. Y. Elec. Rev.*, May 19, p. 1; *Science*, May 25, p. 247; *Prac. Elec.*, May, p. 227.

Progress of the Phonograph. *Sci. Am.*, May 26, p. 320.

Visits to the Phonograph at the Electric Club.—The Phonograph and Music. *N. Y. Elec. Rev.*, May 26, p. 8.

Instruments for Recording and Reproducing Speech. *Science*, May 25, p. 248. (Bell.)

ELECTRIC LIGHT.

The New England Electric Exchange. *Mod. Lt. & Ht.*, May 24, p. 548.

Fire and Life Risks in American Electric Light Installations. *Indust.*, May 25, p. 524. (Soc. Tel. Engrs. and Elecns.)

London Fire Regulations. *West. Elecn.*, May 26, p. 262. (Soc. Tel. Engrs. and Elecns.)

The Relation of Electric Lighting to Fire Insurance. S. E. Barton. *Lond. Elec. Engr.*, May 25, p. 494. (Pittsburgh E. L. Convention.)

The Risks of Fire Incidental to Electric Lighting. (Ill., W. H. Preece. Soc. Tel. Engrs. and Elecns. *Lond. Elecn.*, May 18, p. 56. May 25, p. 91. *Lond. Elec. Engr.*, May 18, p. 472; May 25, p. 502. *Lond. Elec. Rev.*, May 18, p. 584; May 25, p. 624. *Engg.*, May 18, p. 489. (Mascart. *Eng.*, May 18, p. 493; May 25, p. 519. *Indust.*, May 18, p. 504.

The National Electric Light Association. *Mod. Lt. & Ht.*, May 10, p. 503.

Drummers, Gas and the Electric Light. *Mod. Lt. & Ht.*, May 3, p. 485.

The Policy of Gas Companies as to Incandescent Electric Lighting. I. W. Day, Ohio Gas Assn. *N. Y. Elec. Engr.*, May, p. 217. *Lond. Elec. Engr.*, May 4, p. 417.

The Future Prospects of the Gas Interests. Thos. Wood. *N. Y. Elec. Engr.*, May, p. 216. (O. Gas Lt. Assn.)

Electric Lighting. *Lond. Elec. Engr.*, May 4, p. 418. (Paris.) May 11, p. 450.

The Electric Light and Gas Companies. (Note) *Lond. Elecn.*, May 11, p. 5.

Electric Light vs. Gas. *Lond. Elecn.*, May 11, p. 4. (H. Willmott on Billiard Rooms.)

The Application of Electricity to Lighting and Working. W. H. Preece. Lecture I. (Cont'd from p. 693.) *Lond. Elecn.*, May 4, p 736; May 11, p. 9; May 18, p. 41; May 25, p. 83. (Juvenile Lectures.)

Elementary Testing Suitable for Electric Lighting Systems. (Ill.) Thomas Gray. (Cont'd from p. 296.) *Lond. Elec. Engr.*, May 4, p. 414; May 18, p. 463.

The Edison Glow Lamp Patents. *Indust.*, May 4, p. 452. (Cont'd from p. 428.) May 11, p. 476; May 25, p. 525.

In Favor of the United States Company. *Elec. World*, May 26, p. 269. Decision by Judge Wallace in the Edison suits. The Judge practically declines to interfere, compelling defendant to answer, and leaves unchanged previous rulings to the effect that American patents are limited by the terms of like foreign patents.

Important Decision in the Edison Electric Light Suits. *Sci. Am.*, May 26, p. 320.

The Decision of Judge Wallace in the Electric Light Suits. *N. Y. Elec. Rev.*, May 26, p. 2.

Edison and Swan Electric Light Company vs. Holland and others. (Cont'd.) *Lond. Elecn.*, May 4, p. 738; May 11, p. 14; May 18 p. 49; May 25, p. 77. *Lond. Elec. Rev.*, May 4, p. 480; May 11, p. 513; May 18, p. 539; May 25, p. 563.

New Devices for Series Incandescent Lighting. (Ill.) *Md. Lt. & Ht.*, May 3, p. 486; *N. Y. Elec. Rev.*, May 5, p. 8.

Incandescence Filaments. *Md. Lt. & Ht.*, May 3, p. 483.

The Seel Incandescent Lamp Filament. *Elec. World*, May 26, p. 267.

The Schuyler Series Incandescent Lamp. (Ill.) *Elec. World*, May 19, p. 257.

The New Bernstein System of Series Incandescent Lighting. (Ill.) *Elec. World*, May 5, p. 227. (Bernstein Elec. Lt. Mfg. Co.)

A New Incandescent Lamp Socket. (Ill.) *West. Elecn*, May 12, p. 232.

Experiments in Static Electricity with the Incandescent Lamp. (Ill.) E. E. E. Emmons. *Sci. Am*, May 19, p. 308.

Incandescence Filaments. *Lond. Elec. Engr.; N. Y. Elec. Rev.*, May 19, p. 3.

The Haynes Double-Pole Incandescent Switch. (Ill.) *N. Y. Elec. Rev.*, May 5, p. 6.

The Maximum Efficiency of Incandescent Lamps. (Ill.) J. W. Howell. Am. Ins. Elec. Engrs. *Lond. Elecn.*, May 4, p. 754; *Lond. Elec. Engr.*, May 4, p. 425; *N. Y. Elec. Engr*, May, pp 203, 229.

On "Flashing" Carbon Filaments at Different Temperatures. (Ill.) Leigh S. Powell. *Lond. Elec. Rev.*, May 4, p. 468; May 11, p. 500; May 18, p. 532; May 25, p. 561.

Carbons for Arc Lamps. *Indust.; Elec. World*, May 5, p. 227. (Hardtmuth.)

Dimensions of Arc Carbons. *Revue Scientifique; Elec. World*, May 5, p. 226.

The Hill Electric Lighting Apparatus. (Ill.) *West. Elecn.*, May 12, p. 231.

E. M. F. of the Voltaic Arc. *N. Y. Elec. Rev.*, May 26, p. 9.

The Recent Electrical Work of W. S. Hill. (Ill.) *N. Y. Elec. Rev.* May 12, p. 9.

Van Depoele New Electric Lighting System. (Ill.) *West. Elecn.*, May 5, p. 221.

Search Lights with Automatic Feed. (Note.) *Lond. Elecn.*, May 4, p. 730.

Simple Arc Regulator. (Note.) *Comptes-Rendus; Lond. Elec. Engr.*, May 11, p. 436. (Pollak.)

High Resistance Arc Light Carbons. (Note.) *Elec. World; Lond. Elec. Rev.*, May 25, p. 474.

Chicago Arc Light and Power Company Central Station. (Ill.) *West. Elecn.*, May 5, p. 217.

Central Station Lighting in America. (Note.) *Lond. Elec. Rev.*, May 11, p. 520. (Philadelphia.)

Electric Light Installation at the "Scotsman" Offices. (Ill.) *Indust.*, May 18, p. 504.

Electric Light in the Suez Canal. (Note.) *Indust.*, May 11, p. 481. (Corr.) Frederick Walker. *Indust.*, May 18, p. 500.

Electric Light Installation at Hamburg. *West. Elecn.*, May 12, p. 232.

Electric Lighting in the Sandwich Islands. *Md. Lt. & Ht.; Lond. Elecn.*, May 18, p. 32; *Md. Lt. & Ht.*, May 3, p. 484; *N. Y. Elec. Rev.*, May 5, p. 3.

The Electric Lighting of Ems. *Lond. Elecn.*, May 25, p. 86. (Abs. of foreign papers. Inst. Civ. Engrs.)

Prof. Blyth on Wind Power for Electric Lighting. (Note.) *Lond. Elecn.*, May 18, p. 58. (Glasgow Phys. Soc.)

Describes a small plant including storage batteries by which wind power is utilized to supply electric lighting with ten 8 c. p. lamps.

Central Station Lighting in Germany. (Note.) *Lond. Elec. Rev.*, May 18, p. 546.

Diagram of Public and Private Lighting in Berlin. (Ill.) Killingworth Hedges. *Lond. Elec. Rev.*, May 11, p. 502.

Lighting Trains by Incandescent Lamps. *Indust.; West. Elecn.*, May 5, p. 222.

Electrically Lighting Railway Stations. (Note.) *Lond. Elec. Rev.*, May 18, p. 545.

Central Station Lighting. Transformers vs. Accumulators. (Resumed Discussion on Mr. Crompton's paper, from p. 698.) *Lond. Elecn.*, May 4, p. 749. (Soc. Tel. Engrs. and Elecns.) May 25, p. 88. *Lond. Elec. Engr.*, May 4, p. 428; May 11, p. 451; May 25, p. 500. *Lond. Elec. Rev.*, May 4, p. 489; May 25, p. 577. *West. Elecn.*, May, 5, p. 225; May 12, p. 240; May 19, p. 251. *Science*, May 11, p. 226. *Indust.*, May 4, pp. 452, 455; May 25, p. 518.

Electric Lighting from Central Stations. R. E. B. Crompton. *Lond. Elec. Engr*, May 18, p. 474. (Soc. Arts.)

Electric Lighting from Central Stations. (Ed.) *Lond. Elec. Rev.*, May 25, p. 557; *Eng.*, May 4, p. 438.

An Early Inventor in Electric Lighting. *Pall Mall Gazette; Sci. Am.*, May 19, p. 308.

Intensity and Consumption of Different Sources of Light. *Science*, May 4, p. 211.

Petroleum, arc, incandescent, magnesium.

Address by Mr. Frank Ridlon before the Board of Trade, Woburn, Mass. *Md. Lt. & Ht.*, May 24, p. 554.

A Unit of Illumination. *Lond. Elec. Engr.; Md. Lt. & Ht.*, May 17, p. 529.

Photometry. *Lond. Elec. Engr.; Md. Lt. & Ht.*, May 10, p. 508.

Cunynghame's Magnetic Cut-Out. (Ill.) *Elec. World*, May 26, p. 267.

Formula for Wiring. Elmer E. E. Emmons. *Elec. World*, May 12, p. 241. *Lond. Elec. Engr.*, May 25, p. 497.

Testing Electric Light Circuits. (Ill.) *N. Y. Elec. Rev.*, May 5, p. 10. (Rudd, Chic. Elec. Club.)

Artistic Fittings. (Ill.) *Lond. Elec. Engr.*, May 25, p. 486. (Laing, Wharton & Down.)

Diagram Showing the Size of Conductors for House Wiring. (Ill.) C. H. Crockett. *Elec. World.; Lond. Elec. Engr.*, May 18, p. 465.

Double and Single Earth Wire System for Electric Lighting. (Ill.) J. D. F. Andrews. *Lond. Elec. Engr.*, May 11, p. 438.

Switches for Connecting and Disconnecting Lamps at Various Points in the Circuit. (Ill.) *Lond. Elecn.*, May 11, p. 20 (United Elecl. Eng. Co.)

Westinghouse Fittings Exhibited by Prof. Forbes. (Ill.) *Lond. Elecn.*, May 4, p. 752.

Gill's Electric Light Switch. (Ill. Note.) *Lond. Elec. Rev.*, May 11, p. 523.

DYNAMOS, SYSTEMS OF DISTRIBUTION, ETC.

The Eickemeyer Dynamo. *Science*, May 4, p. 212.

Fifty Kilowatt Goolden-Ravenshaw Dynamo. (Ill.) *Indust.*, May 25, p. 522.

A New Steam Dynamo. (Ill.) *Elec. World*, May 5, p. 225. (R. H. Mather.) *N. Y. Elec. Rev.*, May 5, p. 1; *Lond. Elec. Engr.*, May 18, p. 404.

An arrangement by which the engine is entirely enclosed within the dynamo; a three cylinder engine being shown in an armature of Gramme type, admitting and exhausting steam by the hollow shaft.

The "Leeds" Dynamo. (Ill.) *Indust.*, May 25, p. 518; *Lond. Elec. Rev.*, May 25, p. 57[illegible].

Mather and Platt's Dynamos. (Note.) *Lond. Elec. Engr.*, May 25, p. 485.

The Waterhouse Self Regulating Dynamo. (Ill.) *West. Elecn.*, May 12, p. 232; *Elec. World*, May 12, p. 244; *N. Y. Elec. Rev.*, May 12, p. 3.

The Waterhouse Incandescent Dynamo. (Ill.) *West. Elecn.*, May 19, p. 248.

A Novel Dynamo and Accessories. (Ill.) *Elec. World*, May 12, p. 243. (W. S. Hill.)

The No. 1 Plating Machine. (Ill.) *Pr. Elec.*, May, p. 234.

A New Inductorium. (Ill.) *Md. Lt. & Ht.*, May 17, p. 528. (Ries.)

Experiments on Induction Coils. III. (Ill.) Dr. Louis Duncan, Cary T. Hutchinson and Gilbert Wilkes. *Elec. World*, May 19, p. 252.

The Tesla System of Distribution and Electric Motors for Alternating Currents. (Ill.) *N. Y. Elec. Rev.*, May 12, p. 1; *Science*, May 18, p. 238; *Lond. Elec. Engr.*, May 25, p. 403.

A New System of Alternating Current Motors and Transformers. (Ill.) Nikola Tesla (Am. Inst. Elec. Engrs.) *Md. Lt. & Ht.*, May 31, p. 570. Note by Dr. Louis Duncan. *N. Y. Elec. Rev.*, May 26, p. 4.

Electro-Dynamic Rotation by Means of Alternating Currents. (Ill.) *Lond. Elecn.*, May 25, p. 86. (Ferraris.) *Indust.*, May 18, p. 505.

By means of the combination of two alternating fields, displaced by a quarter phase, the practical equivalent of a rotating field is obtained.

Notes on Alternate Currents. (Ill.) J. A. Fleming. Continued from p. 654. *Lond. Elecn.*, May 4, p. 734; May 25, p. 74.

A New Alternating Current Dynamo. (Note.) *Lond. Elec. Rev.*, May 25, p. 574.

A Study of Alternating Current Generators and Receivers. (Ill.) *Md. Lt. & Ht.*, May 24, p. 549. (Anthony, Soc. of Arts.)

The Continuous Current, Limited, vs. The Alternating Current, Unlimited. Geo. Cutter, Chic. Elec. Club. (Ill.) *West. Elecn.*, May 12, pp. 234, 239.

Comparative Value of the Converter and Continuous Current Systems of Electrical Distribution. (Chic. Elec Club.) E. P. Warner. *N. Y. Elec. Engr.*, May, p. 222.

Alternating vs. Continuous Currents. M. M. M. Slattery. (Chic. Elec. Club.) *N. Y. Elec. Engr.*, May, p. 220,

Continuous Current Transformation. (Ill.) R. Percy Sellon, *Lond. Elecn. Elec. World*, May 5, p. 228.

Continuous Current Transformation. (Ill.) R. Percy Sellon. Article II. (Continued from p. 624.) *Lond. Elecn.*, May 4, p. 733.

Edison's High Economy Converter System. (Ill.) *N. Y. Elec. Rev.*, May 26, p. 9.

Self-Induction. (Corr.) W. E. Sumpner. *Lond. Elecn.*, May 4, p. 762.

Variation of Coefficients of Induction. *Science*, May 18, p. 238. (Sumpner.)

Induction. W. E. Sumpner. *Sci. Am. Supp.*, May 19, p. 10,319.

Cost of the Production of Electric Energy. *Centralblatt fur Elektrotechnik*, May 19, p. 309.

The Direct Optical Projection of Electrodynamic Lines of Force and other Electrodynamic Phenomena. (Ill.) J. W. Moore. *Sci. Am. Supp.*, May 5, p. 10,286,

On the Proper Shape for Armature Cores in Dynamo-Electric Machines. W. L. Hooper. *N. Y. Elec. Engr.*, May, p. 201.

Data for Winding Coils. (Ill.) Schuyler S. Wheeler. *N. Y. Elec. Engr.*, May, p. 196.

Includes a table of spaces occupied by wires of different sizes, with single cotton insulation, together with data of the copper.

The Condition of Self-Excitation in a Dynamo Machine. *Lond. Elecn.*, May 18, p. 43. (Thompson, Phys. Soc.) (Note.) The Conditions of Self-Regulation in a Constant Potential Dynamo Machine, May 18, p. 44; *Lond. Elec. Engr.*, May 18, p. 473; *Lond. Elec. Rev.*, May 18, p. 555; *Eng.*, May 18, p. 500.

Experiments Illustrating the Principle of the Dynamo. (Ill.) Geo. M. Hopkins. II. *Lond. Elec. Engr.*, May 4, p. 422.

How Electricity is Generated in a Dynamo Electric Machine. W. S. Hill. *Md. Lt. & Ht.*, May 24, p. 547.

The Turbo-Electric Generator at Newport, R. I. (Ill.) *Elec. World*, May 19, p. 255; *Science*, May 25, p. 247.

The Clarke, Chapman & Parsons combination of a steam turbine or rotary engine directly with a dynamo. Speed of from 9,000 to 10,000 revolutions per minute obtainable.

A Practical Method of Calculating and Designing Dynamos and Motors. F. B. Crocker. *Prac. Elec.*, May, p. 230. (N. Y. Elec. Soc.)

ELECTRIC POWER.

Electric Motors and the Steam Engine. *Am. Machinist; St. Ry. Jour.*, May, p. 129.

The Electric Motor in a Newspaper Office. (Ill) *Md Lt. & Ht.*, May 31, p 571; *N. Y. Elec. Rev.*, May 12, p. 2. (*Post-Dispatch, St. Louis.*)

An Electric Pump. (Ill.) *Elec. World*, May 19, p. 257. (Hall.)

Cotton Printing by Electric Power. (Ill.) *Indust.; Elec. World*, May 19, p. 253.

Mather-Platt motor shown at Manchester (Eng.) Exhibition driving a large color printing machine for calico works.

The Electric Motor in 1849. *Elec. World*, May 5, p. 225. (Patent Office Report.)

A Western Flour Mill Operated by Electric Motors. (Ill.) *Elec. World*, May 5, p. 225. (Laramie, Wyo. With Sprague motors.)

Electric Incline Mining Hoist. *West. Elecn.*, May 26, p. 265; *N. Y. Elec. Rev.*, May 26, p. 3.

Motors and Alternating Currents. (Ill.) Elmer A. Sperry, *West. Elecn.*, May 26, p. 264.

A Commutator for the Simple Electric Motor. (Ill.) (Corr.) G. J. Thomas. *Sci. Am.*, May 26, p. 325.

Pumping by Electricity. *N. Y. Elec. Rev.*, May 19, p. 9.

Phoenix Building Motor System. *West. Elecn.*, May 5, p. 220.

Electricity for Increasing the Tractive Adhesion of Locomotives and Motor Cars. *Lond. Elec. Engr.*, May 18, p. 471; *Prac. Elec.*, May, p. 236. (Ries method.)

Electric Traction-Increasing Device. (Ill.) *St. Ry. Jour.*, May, p. 119.

Electrical Street Railways in England. *St. Ry. Jour.*, May, p. 121.

Electricity Does the Business. *St. Ry. Jour.*, May, p. 127.

Electrical Traction. *Science; St. Ry. Jour.*, May, p. 136.

Pullman Electric Railway. *St. Ry. Gaz.*, May, p. 62.

Whipple's Underground Conduit for Electric Railways. (Ill.) *St. Ry. Gaz.*, May, p. 64.

Electrical Locomotion. Radcliffe Ward. *Lond. Elec. Rev.; St. Ry. Gaz.*, May, p. 69.

Electric Street Cars in Baltimore. *Science*, May 11, p. 227.

The Ansonia, Derby and Birmingham (Conn.) Electric Street Railway. *Md. Lt. & Ht.*, May 31, p. 572.

A New Electric Motor for Tramcars. *Elec. World*, May 12, p. 244. ("Helios" Electrical Company.)

The Carbondale, Pa., Electric Road. (Ill.) *Elec. World*, May 12, p. 244.

Road five miles long, operated by Sprague motors, with overhead system.

Electricity in Place of Horses. *N. Y. Sun; Sci. Am.*, May 26, p. 330.

The Bentley-Knight Electric Railway in Allegheny City. (Ill.) *Elec. World*, May 19, p. 255; *Science*, May 25, p. 248.

The Observatory Hill Passenger Railway of Allegheny, Pa. (Ill.) *N. Y. Elec. Rev.*, May 19, p. 8.

The Sprague Electric Railway System. (Ill.) *St. Ry. Gaz.*, May, p. 63 ; *Prac. Elec.*, May, p. 229.

Traffic of the Richmond Union Passenger Railway Co. *St. Ry. Jour.*, May, p. 121.

Tracks and Conductors of the Richmond Electric Railway. (Ill.) *St. Ry. Jour.*, May, p. 116.

Storage Battery a Practical Success. (Corr.) C. B. Clegg. *St. Ry. Jour.*, May, pp. 122, 123.

Storage Batteries on the Brussels Tramways. *Science*, May, 4, p. 212.

Accumulators versus Direct Current for Electric Traction. (Ill. R. M. Hunter. *Elec. World*, May 12, p. 240. (W. Lineff. Abst. from *Proc. Inst. Civ Engrs*, London.)

Electrical Tramway in Hamburg. (Ill.) J. L. Huber. *Lond. Elec. Engr.*, May 25, p. 487. (Ins. Civ. Engrs.)

Why is Electricity not Used for Street Car Propulsion ? Sidney H. Short. *St. Ry. Jour.*, May, p. 114 ; *Lond. Elec. Engr.*, May 18, p. 466.

Motors for Naval and Military Use. Lieut. B. A. Fiske. *Army & Navy Jour.; Lond. Elec. Engr.*, May 25, p. 499.

Motor for Submarine Boat. (Note.) *Revue Internationale ; Lond. Elec. Engr.*, May 11, p. 437.

Designing Electric Motors. Frederick Walker. *Mech. World ; N. Y. Elec. Engr.*, May, p. 218.

The Transmission of Electric Power. (Ill.) Herr Coerper. *St. Ry. Gaz.*, May, p. 66.

Electricity in Mines. (G. W. Mansfield.) *Eng.*, May 18, p. 476. (Am Ins. Min. Engrs.)

Electrical Tramways: The Bessbrook and Newry Tramway. Ed. Hopkinson. (Cont'd from p. 402.) *Lond. Elec. Engr.*, May 4, p. 423.

Report upon Jarman's Electric Tramcar. (Note.) *Lond. Elecn.*, May 18, p. 38. (Corr.) A. J. Jarman. May 25, p. 97. *Lond. Elec. Engr.*, May 18, p. 472. (Corr.) May 25, p. 491.

The Short Electric Railway at Columbus, O. (Ill.) *Elec. World*, May 12, p. 241. (S. H. Short.)

The Short Series System of Electrical Railways. (Ill.) *West. Elecn.*, May 19, p. 246 ; *St. Ry. Jour.*, May, p. 113.

Sprague Electric Railway and Motor Company's Cars. (Ill.) *West. Elecn.*, May 12, p. 233.

BATTERIES, ELECTROLYSIS, &c.

The Possibilities and Limitations of Chemical Generators of Electricity. F. B. Crocker. Am. Ins. Elec. Engrs. *Elec. World*, May 26, p. 271 *Md.* ;*Lt. & Ht.*, May 31, p 573.

The Samson Battery. (Ill.) *Prac. Elec.*, May, p. 233.

Improved Leclanche Battery. (Ill.) *Elec. World*, May 5, p. 226. (C. J. Hirlimann.)

Combination Grenet Battery and Motor. (Ill.) *Elec. World*, May 5. p. 226. (O. Kanda.)

The Skrivanow Primary Battery. *West. Elecn.*, May 12, p. 239.

"C. & C." Gravity and Storage Battery. (Ill.) *N. Y. Elec. Rev.*, May 5, p. 3.

Effect of Chlorine on the Electromotive Force of a Voltaic Couple. G. Gore. *Lond. Elec. Engr.*, May 18, p. 470. (Roy. Soc.)

Effect of Chlorine on the Electromotive Force of a Voltaic Couple. Dr. G. Gore. (Roy. Soc.) *Lond. Elecn.*, May 11, p. 10.)

Electroplating with Aluminum. *Sci. Am.*, May 19, p. 309.

Electrolytic Bleaching. *Indust.*, May 4, p. 456. (Soc. Chem. Ind.)

Electrolytic Bleaching. *Lond. Elec. Engr.*, May 11, p. 442.

Discussion of methods in vogue for bleaching linen yarns.

The Electrolysis of Copper Sulphate in Standardizing Electrical Instruments. (Ill.) A. W. Meikle. (Phys. Soc.) *Elec. World*, May 5, p. 229.

Tinning by Simple Immersion. *l'Echo des Mines et Metallurgie; Sci. Am.*, May 19, p. 309.

An Electrolytic Indicator of Polarity. (Ill.) *N. Y. Elec. Rev.*, May 5, p. 1.

The Separation of the E. M. F. of Cells into their Potential Differences. *N. Y. Elec. Rev.*, May 5, p. 7.

The Difference of Potential Between Certain Metals, &c., in Solutions of Different Strengths. *Lond. Elecn.*, May 18, p. 56; (*Journal City and Guilds of Lond Inst.*)

Accumulator Tests. *Science*, May 25, p. 248.

Accumulator Tests. (Note.) *Lond. Elecn.*, May 4, p. 730. (Waldenhofen.)

A New Form of Secondary Battery. J. D. Dallas. *Elec. World*, May 5, p. 224. (The Tinol cell.)

Negatives formed of tinned iron wire netting; positives of pure porous electrolytic copper, wrapped in vegetable parchments. The whole is immersed in a solution of zincate of potash, or soda, in a tin box.

New Form of Accumulator. *N. Y. Elec. Rev.*, May 26, p. 9.

Accumulator Regulation. (Ill.) *Lond. Elec. Rev.*, May 25, p. 560.

MECHANICAL.

The Britannia Company's Lathes. (Ill.) *Lond. Elec. Engr.*, May 25, p. 497.

Priestman's Petroleum Engine. (Ill.) *Lond. Elec. Engr.*, May 4, p. 421; *Lond. Elec. Rev.*, May 4, p. 474; *Lond. Elecn.*, May 4, p. 764; *Science*, May 25, p. 247; *Eng.*, May 18, p. 481; *Indust.*, May 18, p. 490.

A New Compound Condensing Machine. *N. Y. Elec. Rev.*, May 5, p. 3.

New Automatic Cut-Off Engine for Electric Lighting. (Ill.) *Elec. World*, May 5, p. 230. (Erie.)

THERMO-ELECTRICITY.

Thermopiles. (Corr.) (Ill.) F. Jehl, *Lond. Elecn.*, May 18, p. 62.

E. Raub's Thermo-Electric Battery. G. Betz. (Ill.) *Sci. Am. Supp.*, May 12, p. 10303.

UNDERGROUND WIRES.

The Philadelphia System for Underground Electric Wires. *Sci. Am.*, May 26, p. 326.

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The Electricity of the Human Body. Dr. R. Vigouroux. *L'Electricité*: *Md. Lt. & Ht.*, May 17, p. 527.

New Electrical Appliances. (Ill.) *Md. Lt. & Ht.*, May 10, pp. 506, 509.

The Radiograph. (Ill.) *Elec. World*, May 5, p. 224. Louis Olivier. *Science*, May 4, p. 212.

A radiometer the speed of which is exactly proportional to the intensity of the light; and the number of revolutions during a given time serve to indicate a definite amount of light effect. In this way the exposure of sensitive plates in photography can be timed.

Practical Analogies between Mechanical and Electrical Engineering. (Corr.) R. M. Hunter. *Elec. World*, May 5, p. 228. Watts. May 19, p. 253.

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On Death by the Electric Current. E. J. Houston. Proc. Am. Phil. Soc. *Elec. World*, May 19, p. 256.

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Non-Magnetic Watches. J. Haswell. *Prac. Elec.*, May, p. 235.
Interesting details regarding their manufacture at Geneva, Switzerland.

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A New Insulating Compound. *Chronique Industrielle; N. Y. Elec. Rev.*, May 5, p. 7.

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On the Heating Effects of Electric Currents. W. H. Preece. (Roy. Soc.) *Lond. Elec. Engr.*, May 25, p. 459; *Lond. Elec. Rev.*, May 11, p. 506; *N. Y. Elec. Engr.*, May, p. 218. (Cailletet.) *Science*, May 18, p. 238; *Elec. World*, May 19, p. 256; *Mf. Lt. & Ht.*, May 19, p. 507.

Giving tables showing the current in amperes required to fuse wires of various sizes and materials, and the diameters of wires of various materials which will be fused by a current of given strength.

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The Glaeser Influence Machine. (Ill.) *Lond. Elecn.*, May 18, p. 45.
Intended to be used for freeing the air from dust and other impurities.

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Lightning Discharges. (Corr.) W. E. Sumpner. *Lond. Elecn.*, May 18, p. 61.

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An instrument devised by the Duc de Feltre, intended to indicate the direction of any distant sound. It comprises a microphonic plate of peculiar construction fixed in a vertical plane, in circuit with a battery and a telephone receiver or a Deprez-D'Arsonval galvanometer and a Wheatstone bridge.

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The Richmond Union Passenger Railway. (Ill.) *St. Ry. Jour.*, June, pp. 143, 156.

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Practical Electrical Units. W. B. Esson. (Concl'd from p. 321.) *Lond. Elec. Engr.*, June 8, p. 535.

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Geyer & Bristol's galvanometer based on the heating effect of the current on a compound bar, consisting of two strips of German silver of different size.

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A Simple Electroscope. *Elec World*, June 23, p. 329.

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Novel Current Registering Instrument. *Science*, June 15, p. 284. (Thomson.)

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On the Maximum Galvanic Polarisation of Platinum Electrodes in Sulphuric Acid. *Lond. Elecn.*, June 15, p. 185.

A Constant Battery in which the Negative Electrode is Carbon. M. M. Faringi & Farkas. *Lond. Elec. Rev.*, June 15, p. 650.

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A Galvanometric Battery. (Ill.) *Engr.; Elec. World*, June 16, p. 302. (Cooke.)

Electrostatic Attraction of Electrodes in Water and in Weak Solutions. *N. Y. Elec. Rev.*, June 23, p. 2.

A New Constant Cell. *Lond. Elecn.; Mod. Lt. & Ht.*, June 28, p. 670.

Magnesium in Primary Batteries. *West. Elecn.*, June 30, p. 320.

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The "Voltaic" Balance. G. Gore. *Lond. Elec. Engr.*, June 29, p. 612.

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The Minimum Point of Change of Potential of a Voltaic Couple. G. Gore. *Lond. Elecn.*, June 22, p. 210.

Effects of Different Positive Metals, &c., upon the Changes of Potential of Voltaic Couples. G. Gore. *Lond. Elec. Engr.*, June 29, p. 612.

On the Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid. G. Gore. *Lond. Elecn.*, June 29, p. 247.

Possibilities and Limitations of Chemical Generators of Electricity. (Ill.) Francis B. Crocker. Am. Inst. Elec. Engrs. *Lond. Elec. Engr.*, June 8, p. 544; *N. Y. Elec. Engr.*, June, p. 248; *Lond. Elec. Rev.*, June 1, p. 624; *Science*, June 1, p. 262.

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Clark Standard Cells. (Note.) *Lond. Elecn.*, June 29, p. 250.

Clark Standard Cells. Durand Woodman. *Elec. World*, June 23, p. 317.

The Samson Primary Battery. (Ill.) *Elec. World*, June 16, p. 307; *N. Y. Elec. Rev.*, June 16, p. 8.

Gassner's Dry Battery. (Ill.) *Lond. Elec. Engr.*, June 29, p. 608; *Lond. Elecn.*, June 29, p. 245.

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Experiments on Electrolysis. *Elec. World*, June 30, p. 333. (Lond. Phys. Soc.)

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Alternate Current Electrolysis. *Comptes Rendus; Lond. Elecn.*, June 29, p. 237.

The Electrolysis of Mercury. (Corr.) F. Higgins. *Lond. Elecn.*, June 15, p. 188.

The Electrolysis of Mercury by the Action of an Alternating Current of Electricity. Lowrie-Hall. *Lond. Elecn.*, June 8, p. 157; *Md. Lt. & Ht.*, June 21, p. 640; *Indust.*, June 15, p. 597; *Lond. Elec. Rev.*, June 8, p. 635; *Elec. World.* June 30, p. 333.

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An Improvement in Secondary Batteries. (Sellon.) *Science*, June 15, p. 284.

The Carrière Accumulator *Science*, June 1, p. 263.

Report of Prof. Kohlrausch on the "Tudor" Accumulator. *Lond. Elecn.*, June 15, p. 180; *Science*, June 29, p. 307.

New Types of Storage Cells. (Note.) *Lond. Elec. Engr.*, June 15, p. 556

Accumulator Tests. (Note.) *Lond. Elec. Engr.*, June 1, p. 507. (Prof. von Waldenhofen.)

Secondary Batteries. (Note.) H. F. Bowling. *Lond. Elec. Rev.*, June 29, p. 706.

The Management of Accumulators. (Corr.) Sir. David Salomons, *Lond. Elec. Rev.*, June 22, p. 687.

Secondary Batteries. (Corr.) H. A. Kent. *Lond. Elec. Rev.*, June 1, p. 609.

Electric Storage. (Ill.) P. Atkinson. *West. Elecn.*, June 23, p. 309; June 30, p. 319.

Practical Notes Concerning the Construction, Use and Management of Storage Batteries. A. Reckenzaun. *Lond. Elec. Rev.*, June 22, p. 665.

UNDERGROUND WIRES.

Underground Electric Conductors in Europe and America. G. W. Plympton. Am. Inst. Elec. Engrs. *Lond. Elec. Engr.*, June 15, p. 563; *Lond. Elecn.*, June 8, p. 143; *N. Y. Elec. Engr.*, June, p. 260.

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The Interesting Discussion of the Wire Problem in New York. *N. Y. Elec. Rev.*, June 9, p. 7.

Underground Conduits. (Note.) *Sci. Am.; Lond. Elec. Rev.*, June 29, p. 705.

Laying Underground Conductors. (Ill.) Chas. W. Farquhar. *Lond. Elec. Rev.*, June 1, p. 597.

THERMO-ELECTRICITY.

On some Early Forms of Electric Furnaces. E. J. Houston. *Jour. Fkln. Inst.; Lond. Elec. Rev.*, June 1, p. 505. (Napier's Electric Furnace.) *Jour. Fkln. Inst.*, June, p. 475. (Siemens' Electric Furnace.)

A Note on Raub's Thermo-Battery. *Lond. Elec. Engr.*, June 29, p. 605.

Thermo-Batteries. (Note.) *Lond. Elec. Engr.*, June 29, p. 604.

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Collodion for Testing an Electric Charge. *N. Y. Elec. Rev.*, (Jouravski) June 30, pp. 4, 9.

A New Electric Combination Lock. *Elec. World*, June 9, p. 294.

An Electric Speed Recorder. G. H. Barrus. *N. Y. Elec. Rev.*, June 30, p. 9.

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On Some Phenomena of Imperfect Ground. (Ill.) (Corr.) A. E. Kennelly. *N. Y. Elec. Rev.*, June 30, p. 3.

A New Joint for Electric Fixtures. (Ill.) *N. Y. Elec. Rev.*, June 30, p. 1.

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Experimental Apparatus for Schools, &c. (Ill.) *Elec. World*, June 23, p. 318.

What Shall We Read? Part I. Thos. D. Lockwood. *Elec. World*, June 23, p. 314. Part II. June 30, p. 326.

Carbon in the Sun. *Elec. World*, June 30, p. 326.

Letters for Learners and Unprofessional Readers. H. D. Wilkinson. (Cont'd from p. 48.) *Lond. Elecn.*, June 8, p. 146; June 22, p. 210; June 29, p. 246.

A New Insulating Compound. *Chronique Industrielle; Elec. World*, June 16, p. 304.

Making Insulated Wires for Electrical Uses. (Ill.) *Sci. Am.; West. Elecn.*, June 16, p. 293.

Okonite. *Lond. Elec. Rev.*, June 8, p. 616.

The Insulation of Conductors. (Ed.) *Lond. Elec. Rev.*, June 29, p. 689.

An English Tribute to American Manufacture. (Ill.) *Lond Elec. Rev.; Md. Lt. & Ht.*, June 28, p. 669.

Testing of Earth Resistances (Corr.) (Ill.) J. H. Pomeroy. *Lond. Elec. Rev.*, June 22, p. 688.

Railway Accidents. (Corr.) W. R. Sykes. *Lond. Elec. Rev.*, June 1, p. 611.

Notes on Electrical America. J. Swinburne & R. P. Sellon. (Concl'd from p. 571.) *Lond. Elec. Rev.*, June 1, p. 590.

Special Magneto Bell. (Ill.) *West. Elecn.*, June 30, p. 317. (Ry. Tel. Sup. Co.)

Dynamo Tenders' Hand-Book. V. F. B. Badt. *West. Elecn.*, June 16, p. 296.

Electrical Scale. (Ill.) *West. Elecn.*, June 2, p. 277. (Elec. Advtg. Scale Co.)

Death of Sir Charles Bright. *West. Elecn.*, June 2, p. 269.

The Electric Resistance of Copper at Low Temperatures. *Eng.*, June 22, p. 625.

The Thomson-Houston Electric Company. *Eng.*, June 8, p. 558.

Electricity in the Navy. (Note.) *Indust.*, June 1, p. 554.

An Electric Organ. *Indust.*, June 1, p. 546.

A New Electric Regulator. (Ill.) *Sci. Am. Supp.*, June 30, p. 10,416.

Papillamatous Tumor of the Bladder Demonstrated by Means of Lester's Electrocystoscope. (Ill.) F. N. Otis. *Sci. Am. Supp.*, June 2, p. 10,354. Dr. Max Nitze. June 2, p. 10,353.

Progress of Electrical Science. *Sci. Am*, June 2, p. 340. (Elisha Gray.)

Execution by Electricity. (Ill.) *Sci. Am.*, June 30, p. 407.

Woodhouse and Rawson's Switch-Board. (Ill.) *Lond. Elec. Engr.*, June 1, p. 519.

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Melbourne Centennial Exhibition. *Lond. Elec. Rev.*, June 22, p. 674.

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On Death by the Electric Current. R. M. Hunter. *Elec. World*, June 9, p. 289. E. J. Houston. Abs. Proc. Am. Philos. Soc. June 16, p. 302; June 23, p. 320. *Lond. Elec. Rev.*, June 1, p. 588.

M. D'Arsonval on Dangerous Currents. *Elec. World*, June 16, p. 305.

Note on Electric Shock in Dreams. E. P. Roberts. *Elec. World*, June 2, p. 270.

Delany's Safety Device for Linemen. C. W. S. Crawley & H. Nalder. *Lond. Elecn.*, June 22, p. 209.

Report of tests upon the effect of the device in shunting the current from the human body.

Danger from Electric Wires. *Sci. Am.*, June 9, p. 361.

Protection of the Human Body from Dangerous Currents. P. B. Delany. Am. Inst. Elec. Engrs. *N. Y. Elec. Engr.*, June, p. 244; *Lond. Elec. Engr.*, June 15, p. 573.

An Oil Insulator for Shackles. (Ill.) *Lond. Elecn.*, June 22, p. 207.

Acid or Resin for Wire Joints. (Note.) *Md. Lt. & Ht.*; *Lond. Elecn.*, June 22, p. 211.

Magnetism in Watches. (Ill.) C. K. Giles. *Elec. World*, June 30, p. 331; *Science*, June 29, p. 308; *West. Elecn.*, June 23, p. 308; June 30, p. 320.

Magnetism in Watches. Theo. Gribi. *Am. Jeweler.* *N. Y. Elec. Rev.*, June 9, p. 10; *West. Elecn.*, June 23, p. 311.

Non-Magnetic Balance and Alloys. *N. Y. Elec. Rev.*, June 23, p. 6.

Another Electric Alarm Clock Arrangement. *Elec. World*, June 16, p. 308.

The Eco-Magnetic Clock. (Ill.) *N. Y. Elec. Rev.*, June 16, p. 1.

Court of Patent Appeals. *Md. Lt. & Ht.*, June 14, p. 620.

The Patent Work of the National Electric Light Association. *N. Y. Elec. Rev.*, June 16, p. 2.

The Patent Court and Uniformity in Patent Office Practice. G. H. Stockbridge. Am. Inst. Elec. Engrs. *Elec. World*, June 2, p. 283; *N. Y. Elec. Engr.*, June, p. 257.

The Act of March 3, 1887, and How it Affects Patent Practice. W. D. Baldwin. *Elec. World*, June 16, p. 303.

A Court of Patent Appeals. *West. Elecn.*, June 23, p. 312.

Electrical Patents. *Sci. Am.*, June 23, p. 389.

Lightning Induction in "Bearer" Wires and Other Things. (Corr.) H. M. Sayers. *Lond. Elecn.*, June 15, p. 187.

Saunders' Lightning Guard. *Sci. Am.*, June 16, p. 10,383.

The Protection of Buildings from Lightning. Dr. Oliver Lodge. *Lond. Elecn.*, June 22, p. 204; June 29, p. 234. *Lond. Elec. Engr.*, June 22, p. 594; June 29, p. 613.

Electric Welding. O. K. Stewart. Boston Elec. Club. *Md. Lt. & Ht.*, June 7, p. 602; *West. Elecn.*, June 23, p. 311.

Welding Rails by Electricity. (Corr.) O. K. Stewart. *N. Y. Elec. Engr.*, June, p. 268.

Electrical Welding. *Science*, June 8, p. 273; *Indust.*, June 8, p. 572.

Am. Inst. of Elec. Engrs. *West. Elecn.*, June 9, p. 288; June 16, p. 297; June 30, p. 323.

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Notes on Induced Currents. (Ill.) G. Doyle. *Md. Lt. & Ht.*, June 21, p. 641.

Progress of Electrical Science. Elisha Gray. *N. Y. Elec. Rev.*, June 16, p. 1. Extract from lecture.

Paramagnetism and Diamagnetism. (Corr.) J. A. Ewing. *Lond. Elecn.*, June 22, p. 220.

The Theory of Diamagnetism. *Cosmos; Elec. World*, June 16, p. 306.

The Conductivity of a Vacuum. *Wiedemann's Annalen; Elec. World*, June 23, p. 315; *Lond. Elecn.*, June 1, p. 105; *Science*, June 29, p. 308.

Unipolar Induction. (Ill.) G. W. de Tunzelmann. *Lond. Elecn.*, June 8, p. 139; June 15, p. 171; June 22, p. 203; June 29, p. 232. (Corr.) N. G. Thompson. June 29, p. 250.

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